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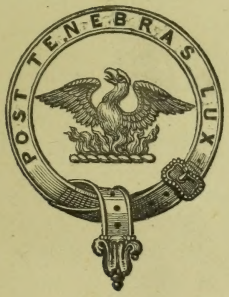

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THE QUARTERLY

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JANUARY, 1877.

I. PHYSICAL CHANGES UPON THE SURFACE OF THE MOON.

By EDMUND NEISON, F.R.A.S.,
Author of "The Moon, and the Condition and Configuration of its Surface."

THE present condition of the surface of the moon is one of the most interesting and important questions within the whole range of Astronomy; and being a subject of considerable difficulty, and requiring a thorough acquaintance with more than one branch of science to be properly dealt with, it is not surprising that widely different views on the question are held by different astronomers. The question is, however, of importance to others besides astronomers, and many eminent physicists and geologists have recognised that the study of the present condition of the surface of our satellite promises to throw much light on many vexed geological and physical problems. The opinion of that eminent geologist the late Prof. Phillips on this subject is well known, for he often expressed his conviction that the lunar surface presented the best field for the study of many of the more difficult problems of geology, and under this conviction he devoted many years to the study of selenography. Considering, in fact, the close relationship between the earth and its satellite, and the general analogy that selenographers maintain exist between their respective classes of primitive formations, it is unquestionable that the study of lunar surface must throw light on many obscure points in the past history of the earth, and probably in fact on the general history of the planets.

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The question of the present condition of the surface of the moon has engaged, therefore, the attention of some of the most eminent astronomers, and much has been done towards obtaining the information necessary for the proper consideration of the problem. But though much has been done in this direction, very much more remains to be done before this question can be held to admit of decisive settlement; for though the general nature of the lunar formations and surface has been made out fairly well, the details, which are of the utmost importance, are only very partially understood. The paucity of material available on which astronomers in general may found an opinion is not widely understood, for, with the exception of a few pages in the great work of Beer and Mädler's "Der Mond," and a small work by another great selenographer (Schmidt, of Athens), also entitled "Der Mond," until lately no published information may be said to exist accessible to astronomers in general. Hitherto information, with regard to the details of the structure of the formations on the surface of the moon, has had to be obtained directly by the careful study of the lunar surface,—a work necessarily occupying years, and only to be acquired by assiduous labour. These particulars, though not at first sight of importance, must not, however, be overlooked, for it will subsequently appear that they go far to explain the position now occupied by the question—What is the present condition of the lunar surface?

It is a remarkable circumstance, in relation to this question, that whereas those astronomers who have devoted much time and labour to the study of the moon's surface, and to whom astronomers in general are mainly indebted for our present knowledge of the surface of our satellite, hold in general one view as to the present condition of the lunar surface, astronomers as a body hold a very different opinion. To take a striking instance,—scarcely any astronomer known to have devoted time to the study of selenography doubts that many processes of actual lunar change are in progress, and it is doubtful if there is one who could not promptly instance one or more such cases. Yet the general opinion of astronomers appears to be against any such physical changes having occurred. And another instance, almost as striking, exists in connection with the subject of the lunar atmosphere: whilst all selenographers appear to have detected instances where the existence of this atmosphere is revealed, astronomers in general appear to question almost the possibility of its existence, and this in face of the absence of any evidence

whatever that there is no atmosphere of the nature supposed.*

It would be an interesting inquiry to ascertain in what manner arose this direct conflict of opinion on this subject, between those who have systematically studied the appearances presented by the moon, and those who have not in the same systematic and assiduous manner examined the lunar surface. It appears to have originated in a short summary by Mädler of the appearances presented by the moon, wherein the differences between the condition of the moon and earth were forcibly stated, and where he pointed out the impossibility of the view that was held in the time of the earlier astronomers,—that the moon might be a mere copy of the earth, containing a dense atmosphere, large oceans, abundant vegetation, and animal life, or even human inhabitants. In a condensed and more unqualified form these remarks crept into all astronomical text-books, and were the main basis of the views commonly held by astronomers.

The present condition of this question is therefore not surprising; with the views of those astronomers who have devoted many years to the study of the moon, but very imperfectly known, and in fact inaccessible to most astronomers, the general body of observers are left to draw their own conclusions from their own acquaintance with the lunar

* The subject of the phenomena which would be presented by the moon were it surrounded by an atmosphere of any given density is one of very considerable difficulty. It cannot be treated in a popular—and therefore necessarily superficial—manner, because it involves subjects demanding a tolerably extensive acquaintance with mathematical physics to enable their real bearing to be understood. The mathematical treatment of the subject, though involving no point of any particular difficulty, is nevertheless complex and very laborious, from its extent. It is not surprising, therefore, that when this subject is regarded in a popular manner ideas should be arrived at that a more elaborate and careful examination shows, by mathematical demonstration, to be mistaken; and it was in this manner that Bessel, in his investigation of a portion of this subject, arrived at results which—though they have been since shown to be wrong—have exerted the greatest influence on the progress of selenography. In saying that there is no real evidence whatever that the moon does not possess the atmosphere which has been ascribed to it by the author, it must not be supposed that no attention has been paid to the various circumstances which have from time to time been pointed out as proving the impossibility of the moon possessing an atmosphere of a surface density far less than this. But on submitting these objections to the careful examination that they demand, and in particular by calculating with the aid of the powers of mathematical analyses what would be the real dimensions of these phenomena which it is supposed would reveal the existence of the lunar atmosphere, it is found that, for the atmosphere supposed, they would be insensible. Most of these objections have been already considered in my recent work on the Moon, or in the "Quarterly Journal of Science" (October, 1874). The whole subject involves such difficulties that statements founded on merely a popular or general consideration of the problem it involves require therefore to be regarded with caution.

surface. Approaching the study of the moon with strongly preconceived notions that the surface is a mere arid desert, nearly red-hot at times and almost immeasurably cold at others, without water, air, or life, the real condition of the moon is not such as to dissipate these views at the first glance. Its cold, still, apparently unchangeable surface, so utterly unlike what the earth might be supposed to appear as seen from the moon, convinces the casual observer that the world he then sees is utterly unlike the world he knows. He looks for immense cloud-masses floating in a dense atmosphere, and sees none; for wave-tossed seas and winding broad rivers, and there are none; searches for luxuriant forests and green prairies, and they are absent. This is enough, and he retires from further contemplation of the "airless, waterless, lifeless, volcanic desert" of the text-books. During subsequent periods he may again take a casual glance of our satellite, look at lunar occultations, or watch the grand changes in appearance that the face of the moon presents as the sun sweeps across its heavens and slowly illuminates in turn its varied formations. But study the surface, endeavour to piece together bit by bit the different features that are revealed, until he thoroughly comprehends the details and nature of the formations he sees, is what the general observer and the great majority of astronomers do not even attempt to do.* To properly become acquainted with the nature of the lunar surface requires that the study of the moon should be made the primary object of one's observations; and those who have done this, and devoted years to the assiduous study of the moon, constitute those astronomers who—as previously mentioned—hold different views as to the present condition of the lunar surface to astronomers in general.

The previous observations are, it will be seen, of importance in connection with the subject of the present article, for they show how selenographers and astronomers in general may hold very different opinions on questions connected with the moon,—one are led to conclusions from the careful study of the minuter details of the lunar forma-

* An interesting instance, illustrating the condition of the general knowledge of selenographical subjects amongst astronomers, is the prevailing opinion that no retardation of occultation of stars by the moon has ever been observed, but that stars approach the limb of the moon, and disappear behind it at the instant they theoretically should. Yet it has been stated repeatedly that the only three series of reductions of the observation of occultation of stars show that a very considerable retardation does take place—a retardation of from 5 to 10, and often even 15 seconds of time. Different opinions may be held as to the cause of this retardation of occultation, but of the fact there can be no question, and it has been known for nearly twelve years.

tions, that the others, not thus acquainted with these features, refuse to accept. The purport of the present paper is to detail instances of what selenographers consider to be real physical changes in the moon, together with some of the reasons which they have for supposing them to be such. From an opposite point of view the subject has been already ably handled in these pages by one of our most eminent astronomers, Mr. R. A. Proctor, who in his own department of the science stands still unrivalled.*

The most prominent instance of a supposed change on the surface of the moon is indubitably that of Linné, because it is an instance which was brought before the entire astronomical world, and engaged for nearly a year the attention of almost every astronomer in Europe. The leading facts of the case may be briefly stated as follows:—On the north-west quadrant of the moon is a large tract of nearly level surface, of a pale grey, almost greenish colour, and this region—which is in extent about 430 miles in diameter—contains near the centre a moderate-sized bright crater, called Bessel, nearly 14 miles in diameter, and with a circular wall rising 4000 feet above the interior and about 1600 feet above the surrounding surface. Scattered about this plain are a few small crater-like objects, about 2½ miles in diameter and with walls rising about 300 feet high. Now, near the eastern centre of this great tract of level ground, or the *Mare Serenitatis* as it is called, one of the most eminent selenographers, Lohrmann, had placed a distinct bright crater, subsequently called Linné, and this crater he described as being about 5 miles in diameter, and, after Bessel, the most conspicuous object on the Mare Serenitatis. Ten years later our greatest selenographer, Baron von Mädler, confirmed Lohrmann's description, and named the crater Linné, which he described as deep and very distinct in oblique illumination. He measured its diameter, which was 6½ miles, or about half the size of the great crater Bessel, and selected it as the most distinct object in its region, for one of the measured units in his great trigonometrical lunar survey. Now, had Lohrmann's account not been correct, Mädler would certainly have been struck at once by the fact; but Mädler's work, which was entirely independent of his predecessor, entirely confirms the account. And had Mädler's account been incorrect, the instance would have been so evident that it could not have escaped the attention of Gruithuisen and Schmidt, who were about this time making

* Quarterly Journal of Science, January, 1873, and October, 1873.

lunar observations, and especially the latter, as he repeatedly drew this particular part of the moon. In the drawings of Schmidt, however, whenever this crater Linné is shown, it is as a deep crater, entirely in accordance with the description of Lohrmann and Mädler.

In October, 1866, however, Schmidt was startled by finding no trace of Linné, when it was in a position when it should have been most conspicuous. Instead of the deep wide crater with its interior filled with intense black shadow, all he could detect was a faint, indistinct, cloudy marking, about 5 miles in diameter. During the subsequent evenings it retained this aspect, though at times it was so indistinct as to be barely visible. Schmidt immediately announced this startling circumstance, and during the end of 1866 and the commencement of 1867 nearly every astronomer in Europe directed his attention to this now-celebrated spot. Since then this portion of the moon has been repeatedly examined by selenographers, but Linné has never been seen of the size and character ascribed to it by Lohrmann, Mädler, and Schmidt. The conspicuous deep crater has utterly disappeared.

In what manner is this change to be explained, and how is it that apparently so unmistakable an instance of physical change has been rejected as not established? These questions are best answered by describing what has been seen in the place formerly occupied by the deep crater. Where Linné was, it has been already mentioned, an indistinct white cloud-like mark was seen; but powerful telescopes and assiduous examination soon revealed something else. When first the sun illuminated the region containing Linné a small conical mountain peak was detected, this mountain being about 100 to 200 feet high, and casting a short black shadow, and was by some mistaken for a small crater. Subsequently other observers detected a minute crater cone. When first seen it was estimated by Schmidt to be about a quarter of a mile in diameter, and by Secchi, of Rome, and Buckingham as about one-third of a mile across; this crater lying to the west of the small hill. If the estimates of these earlier observers are to be trusted, and Schmidt at least was not likely to be mistaken, a most important circumstance must have occurred; for when next seen the crater was nearly three times as large, or nearly 1 mile in diameter, according to D'Arrest of Copenhagen, Schjellerup, Huggins, and Wolf. In July, 1867, the crater was sufficiently large and distinct to permit of its being measured, and according to Huggins it was rather under 2 miles in diameter—a result confirmed,

during the next month, by Buckingham and Knott. It must be remembered, however, that this crater was only seen when the region was very obliquely illuminated by the sun, at sunrise or sunset in fact. At any other period all that was seen was a white cloudy marking, about 8 miles in diameter. Since 1868, it is only on three or four occasions, when the atmosphere has been exceptionally good, that the small crater within Linné has been seen, and then only in powerful telescopes. It does not seem ever to have been seen as a distinct crater in telescopes as small as those employed by the earlier selenographers, who only knew the crater as a fine, distinct, conspicuous object, with a black interior, nearly five times the diameter of that of the present crater. The present diameter of the crater opening of the cone on the site of Linné is about $1\frac{1}{4}$ miles, the total diameter of the cone at the summit about half as much again, and at the base about $2\frac{1}{2}$ miles. The height above the surface is about 200 feet at most.

As a crater, in fact, Linné is now one of the most difficult to see on the entire Mare Serenitatis. Under illumination, when every other crater stands out boldly as distinct objects, Linné is, with rare exception, either invisible as an elevation or else appears as an insignificant hill. It is questionable whether the earlier selenographers could have seen it at all under these conditions. It should be mentioned, however, that between 1867 and 1869 several observers considered that they could detect traces of the ring of a very shallow crater around the crater cone in the position of Linné, and in size slightly larger than the crater of Beer and Mädler. The actual existence of this ring is doubtful; it has not been seen since 1869, and the appearance noticed probably arose from a number of ridges and mounds near the position of Linné.

Considering, then, the facts as stated above, it would appear that there could be no question but that a real physical change had occurred on this portion of the surface of the moon. The large crater independently described by Lohrmann, Beer and Mädler, and Schmidt, as existing in this portion of the Mare Serenitatis, unquestionably no longer exists, and in its place appears a white cloudy marking, containing a small crater cone, with an opening scarcely one-twentieth of the area of the former crater. What reasons have therefore been advanced against any change having taken place, that astronomers in general should favour this view? The principal reason why astronomers are so strongly against allowing that any change can

have occurred appears to be the existence of a strong prejudice against the very possibility of such changes occurring. The probable origin of this feeling has been already adverted to. This of itself, however, would not have been sufficient, of course, to bring about this result; it remains, therefore, to consider the evidence against the reality of any change in Linné having occurred that has from time to time been brought forward.

Immediately after Schmidt had announced the supposed change in Linné, reference was made to the lunar drawings in the "Selenotopographische Fragmente" of Schröter, of Lilienthal, the earliest of the great selenographers. Amongst the very earliest drawings was found one of the Mare Serenitatis, made on November 5th, 1788, with powers of 95 and 161, on a reflector of 6 inches aperture and 7 feet focus. On this drawing Schröter does not draw Linné as a crater, but not very far from its place he draws a white spot on a ridge which he marks *v*, and a larger dark spot which he marks as *g*. Schmidt considered this white spot *v* to be Linné, and this view has been strongly urged by Huggins, and has been generally accepted as correct by astronomers. It has been urged, therefore, that as Schröter drew this formation in a manner not unlike its present appearance no change can have occurred, but that Lohrmann, Beer and Mädler, and Schmidt must all have been entirely mistaken as to what they thought they saw. It was also found that, on reference to a map made by Lahire during the seventeenth century, no trace of Linné is to be found. It may be, however, at once observed that this last fact is absolutely valueless; the map of Lahire is perfectly untrustworthy in these minute details, and Hevelius, Riccioli, and Cassini are little superior. From all the maps made about this date numerous craters are omitted, far larger than Linné ever was supposed to be, so that the fact of Linné not having been drawn proves nothing. These maps were made principally by full-moon drawings, where Linné would not be visible as a crater. Even, however, had this not been so, the value of this negative evidence from Lahire's map is entirely destroyed by the direct evidence furnished by Riccioli's map, which shows Linné as a distinct crater; and it must be remembered that the present crater on the site of Linné could not possibly have been seen by Riccioli with the optical means by which the materials for his map were obtained.

The only real evidence, then, that no change has taken place in Linné is this single drawing of Schröter's, which is

relied on as proving that the subsequent maps and drawings of Lohrmann, Beer and Mädler, and Schmidt must be entirely wrong in this point. It is a remarkable circumstance that in every other case of a discrepancy between the drawings of Schröter and Beer and Mädler, one alone of the above authorities, the drawings of Schröter, have been rejected as entirely unworthy of comparison with those of Beer and Mädler. In this particular instance, however, one of the earliest drawings of Schröter, made with his most imperfect and least powerful instruments, on an occasion when the definition from his own account must have been very inferior, has been brought forward to prove the incorrectness of the drawings and micrometric measures of his great successors. Now, no selenographer acquainted with Schröter's works will allow that this is permissible. Schröter's early drawings are never to be trusted when they differ from his successors in the more minute features. Whenever a discrepancy exists, it will be found that Lohrmann or Beer and Mädler are correct, and Schröter wrong. It is true the same does not hold with Schröter's later drawings, and especially when he was in possession of his great reflectors of 13 feet and 26 feet focus respectively; but this particular drawing of Schröter's is one of his very first. It must also be remembered that Schröter did not draw the portion containing Linné with the same fulness or accuracy as the rest, but that, while the western portion is fairly correctly drawn, the eastern portion, where Linné should be, is misplaced and imperfect. Mr. Birt, our best English selenographer, doubts in fact whether this white spot, *v*, really is meant for Linné, and has made out a very strong case for supposing that the dark grey spot, *g*, farther south, really represents Linné as seen by Schröter. If this is really the case it entirely upsets the view commonly held by astronomers that Schröter's drawing proves no change to have occurred in Linné. For Schröter describes this spot *g*, which was only faintly illuminated, from its being near the dark portion of the moon, as being very similar to, and probably of the same nature as, another smaller formation a little to the west of it. Now this smaller formation, *r*, is really a fine distinct crater, Bessel *m*, about 3 to 3½ miles in diameter and 600 or 700 feet in depth. If, then, the spot *g* is Linné, as seen by Schröter, it would have been a large crater, in tolerable accordance with the description of Lohrmann and Beer and Mädler. Further, as Mr. Birt points out in relation to the western and best shown portion of the drawing of Schröter, *g* falls exactly into the position which Linné should occupy.

For the reasons detailed in the above, therefore, selenographers consider that the attempt to show that Linné has not changed has entirely failed, but that in the case of this crater we have an instance of real physical change on the moon. With regard to the nature of this change little definite has as yet been ascertained, and it will require a long examination of this region with powerful telescopes to determine what change really has occurred. From numerous observations the explanation agreeing best with the present conditions of the surface is that the walls of the old crater have collapsed and fallen into the interior. By this means the interior would be nearly completely filled up, leaving, however, a sort of rough, cone-like, small crater towards the centre. Under exceptionally favourable atmospheric conditions, with the assistance of a powerful telescope,* the surface immediately around the small crater has been seen to present the appearance of being rough and irregular. Round what would have been the border of the old crater are numerous mounds and rough blocks, and on the east is one if not two low hills or peaks, presenting the appearance of being portions of the old wall. The difficulty of making these observations is very great, and they are only possible in the very finest atmospheric conditions. A prolonged examination of this region, however, would amply repay the labour.

There is one other point on which a remark must be made, and that is with reference to an apparently generally spread misconception of the change which Linné is supposed to have undergone. Thus, in the interesting article on this subject in the "Quarterly Journal of Science" for October, 1873, Mr. R. A. Proctor says:—"Mr. Browning, after considering the evidence afforded by his own observations, considered in connection with those made by others, arrives at the conclusion that 'there is scarcely any ground for supposing that any change has occurred in this small but celebrated crater.'" Mr. Proctor then adds—"These remarks appear to me to contain the gist of the whole matter. We see that Linné has a surface so constituted that as the sun is rising there, and so pouring his rays very obliquely, there is a continual change of aspect precisely resembling that which can be recognised when certain kinds of rock surfaces, and especially crystalline formations, are viewed under oblique illumination. We know that in such cases the tints vary, not only absolutely but relatively, insomuch

* With an aperture of 9½ inches, and a power of 600.

that a part which is darker than another with one oblique illumination will be lighter under another and but slightly different illumination.

"It appears to me that no other explanation can reasonably be suggested, because, in point of fact, we have to choose between the theory that there has been a definite change of surface on this part of the moon, or that the change is only apparent. Now, if there has been a definite change at any time, fresh changes must have restored, either from time to time or definitely, the former condition of the surface. But this seems extremely unlikely, whilst such a change as Sir John Herschel considered to afford the best explanation of Schmidt's observation may be regarded as one which no subsequent process could so modify as to restore, or nearly restore, the original appearance of the region." (This explanation of Sir John Herschel's referred to was that the crater of Linné had been filled up to overflow with viscous lava.) "Such a change would doubtlessly account well for the observed appearances, but it leaves the subsequent restoration of the crater unexplained."—(Pp. 500, 501.)

From the above quotation Mr. R. A. Proctor would appear to consider that the entire known observations that have been made of Linné can be explained by supposing the surface to vary in tint with differences of illumination, and he also seems to suppose that of late years Linné has been seen to present the same appearance and possess the same characters as it was described as having before the supposed change occurred. But it has been already shown that this is entirely a misconception: whatever change occurred in Linné was definite, and since 1867 Linné has never been seen to accord with the description of the early selenographers. To suppose the minute crater-like formation now existing on the site of Linné can be in any manner identical with, or even similar to, the twenty times larger formation that was observed, drawn, and measured by Lohrmann, and Beer and Mädler, is out of the question. Nor will any selenographer allow that differences of illumination, of any kind or on any surface, are capable of explaining the difference in appearance of the formation at the two different epochs. No alteration in illumination whatever could make an object where Linné is placed look at one period like a considerable and deep crater, and at another as a small, scarcely visible crater. The means by which Mr. R. A. Proctor would explain the supposed change in Linné are perfectly inadequate. Nor will the effect of variation in illumination even account for the minor variations which it has been supposed have

occurred since 1866, such as, for example, the alteration in the size of the small crater within Linné from a diameter of barely one-third of a mile in January and February, 1867, to a diameter of five times as great in July, 1867. It is true the reality of many of these supposed changes is doubtful, but alterations in the angle of illumination are inadequate to explain them *per se*. In fact, so far as the known observations extend, the alterations undergone by Linné in its present condition appear to be perfectly normal, and exactly similar to the changes undergone by all other analogously placed and constituted formations. The changes are striking, but in no manner strange or unexpected to the experienced selenographer.

The facts about Linné may be therefore summed up very briefly. According to three or more independent selenographers, the most experienced and eminent that Science has seen, the object named Linné was a conspicuous crater of large diameter and great depth. Now in its place all that exists is a tract of uneven ground, containing a small, scarcely visible, insignificant, crater-like object. It is impossible that one could ever be systematically taken for the other. It is inconceivable how our three greatest selenographers could have systematically and independently made the same blunder, and that one blunder only. For in no other case do we find any error of this nature. Their description must therefore be held to truly describe the nature of the formation at their epoch (1820—1845). The object is no longer of the same size and description. A real physical change on the moon's surface must therefore have occurred at this point. This, then, is the conclusion that selenographers as a body have arrived at, yet, despite the strong evidence on which it rests, it is not generally recognised by astronomers.

The next instance of supposed physical change on the surface of the moon is one of the most peculiar in the entire range of astronomical observations, and is that in the case of the crater named Messier by Beer and Mädler. In the equatorial region of the moon, on the *Mare Fœcunditatis*, the westernmost of the great lunar plains, close to one another are two small crater plains, according to Beer and Mädler about 9 miles in diameter. These two formations lie isolated on the open plain, and are surrounded by only some very low ridges, and mounds, and some rounded depressions like craters. From the easternmost of the two, Messier A, extend two long, slightly diverging streaks, of a pale grey colour, which in full moon give the formation

much the appearance of a bright comet with a long double tail. Schröter, of Lilienthal, discovered this object, and drew it carefully, making the western formation of the two —or Messier itself—slightly the larger. He also suspected that its appearance was variable. In consequence of the observation of Schröter, Beer and Mädler paid particular attention to these formations, examining them most carefully on more than three hundred distinct occasions, between 1829 and 1837. They were thus enabled to declare with certainty that the two formations, Messier and Messier A, were exactly alike in every manner, not the slightest difference in any particulars being detectable. Both were circular crater plains, 9 miles in diameter, with 7° bright greyish white walls, surrounding a yellowish grey interior only 3° bright. On the walls, which were of the same height, were several wall peaks, in both formations situated in the same position with regard to the formation. In fact, as Beer and Mädler draw particular attention to, their own observations are decisive proof that the two formations were exactly equal in every respect, and that in diameter, form, height of their walls above the surrounding surface, and depth of the interior beneath the crest of the walls, colour of the interior and of the walls, and the position of the surrounding wall peaks, Messier and its twin formation Messier A were completely alike. And it is impossible that any difference in these respects would have escaped the attention of the greatest selenographer of our day, during a course of over three hundred observations in a space of nine years.

Some years after this, Gruithuisen—a most zealous and keen-sighted, though fanciful, observer—detected a slight dissimilarity between the form of the two craters, but this fact received little, if any, attention. In November, 1855, the Rev. T. W. Webb, one of the best living lunar observers. who was examining this region with a telescope of similar dimensions to that of Beer and Mädler's, observed that the eastern crater plain appeared the larger of the two. On again looking at the formations on March 11th, 1856, he at once detected that not only was the western crater plain, or Messier the smaller of the two, but that it was elliptical, with its greatest diameter extending from east to west. This fact was confirmed by subsequent observations and from drawings made by him in 1857, whilst Messier A, the eastern formation, appears to have remained unchanged, still being a circular crater plain, with a diameter of 9 miles, the western crater plain, Messier itself, had an elliptical

form, with a long diameter of about 10 to 11 miles and a short diameter of about $7\frac{1}{2}$ to 8 miles. The most rigid equality between the form and dimensions of these two formations, so strongly asserted to exist by Beer and Mädler, then no longer existed in 1857, according to the observations of the Rev. T. W. Webb.

This fact was of very great selenographical importance, and should have attracted general attention, because whatever uncertainty may be attached to the observations of Linné made by Beer and Mädler, it cannot be seriously urged that Beer and Mädler could have failed to recognise the dissimilarity between Messier and Messier A discovered by the Rev. T. W. Webb, when we consider that for over three hundred times they most carefully examined the two formations for the express purpose of detecting any difference. Nor can that potent agent, the lunar librations, be invoked to account for this difference, for in this particular instance it is entirely without sensible influence. For Messier lying very close to the moon's equator, and in longitude 47° W., no variation in the libration of the moon can sensibly affect the meridianal apparent diameter of the formation, and it is in this last that the diminution has been observed. If it was established, therefore, that the two formations were no longer alike, but differed markedly in any respect, the very strongest evidence of actual physical change would be the consequence.

Although therefore it is thus important that further observations of these formations should be obtained, the matter attracted little attention until the period 1870 to 1875. During these years Messier and Messier A have been repeatedly examined with the aid of most powerful telescopes, and the present dissimilarity between Messier and Messier A has been placed beyond the possibility of doubt. From some measures during the past year the long diameter of Messier appears to be 12·2 miles, and the short diameter—which is nearly, though not quite meridianal—is 6·9 miles. The difference between the form and dimensions of the two formations is now obvious in the smallest astronomical telescope, for it is unmistakable with a power of 150 on a telescope with an aperture of a little over 2 inches. With the fine Frauenhofer equatorial of Beer and Mädler, with an aperture of nearly 4 inches and a power of nearly 300, the difference now existing between the two formations would have instantly arrested the attention of any selenographer. It is inconceivable that the two formations could have possessed their present form in Beer and Mädler's time, and

have been on hundreds of different occasions carefully scrutinised to detect any difference that might exist between them, without the most marked dissimilarity which now exists being detected.

The most difficult portion of the question remains, however, still untouched; for even were it granted that in the case of Messier there exists an instance of real physical change, the process by which that change could have been brought about seems to defy explanation. In the case of Linné we have a plausible and easy method of accounting for the observed phenomenon, but in Messier what it seems must have occurred is a slow squeezing of an immense crater plain out of shape. The nature of the forces and processes at work upon the surface of the moon, which could compress a circular crater 9 miles in diameter into an elliptical formation some 12 miles by 7, is such with which there at present exists nothing analogous on the surface of the earth; and until some satisfactory theory could be devised which would show by what means this change in Messier could have been brought about, in a manner consistent with other selenographical facts, it is not surprising that a strong reluctance should exist to admitting that a change had occurred in the form and dimensions of Messier, even when backed by the extremely strong evidence in its favour that has been adduced.

It is possible, however, that this difficulty may be avoided, for careful examination of the crater plain Messier and its neighbourhood suggests that, instead of a bodily compression of the entire crater, the observed change may have occurred from the gradual sliding of the north and south walls into the interior, and in so doing pushing the entire western wall, outwards and westwards, down an incline existing there. If this explanation is found to be consistent with the present condition of the formations, it at once removes all the difficulties, because the process it involves—namely, the gradual slipping of the wall of a formation into the interior—is one of which numerous traces exist upon the moon. There would be little difficulty on the part of selenographers in pointing to a hundred instances where a like circumstance had occurred,—one within Gassendi being a notable case, and one where a massive wall has been pushed back nearly 10 miles. As far as is at present known, this explanation is in accordance with the condition of the surface immediately around Messier, but further observations with powerful telescopes are indispensable. Unfortunately the region of the moon containing Messier is one which cannot often

be observed at the proper time for this purpose ; so that only one or two days during the year are likely to be found when Messier is in a proper position to be observed, and the weather will permit a high power and a powerful telescope to be used to advantage, for these require very steady and favourable atmospheric conditions.

It is not, however, in these two instances alone that selenographers have detected instances of changes in the lunar surface ; but these two instances are cases where there exist the very strongest evidence that real physical changes have occurred. There are various other remarkable cases which render it very probable that different small portions of the moon's surface have undergone striking variations ; but though they rest on evidence that in other branches of astronomy have been usually accepted as sufficient to establish the existence of such variations, they do not rest on evidence of so overpowering a weight as is now alone considered worthy of credit on these selenographical questions. Thus towards the central portions of the moon, on the wide open plain called the *Mare Nubium*, according to Beer and Mädler, when they drew their map and whilst they were making their observations, there existed a fine, deep, glittering white crater, called by them *Alpetragius d*. It had a diameter of 5 miles, and close to it, on the south-west, was a smaller crater, about 1 mile in diameter. The large crater Alpetragius *d* has entirely disappeared, leaving in its place a slightly less bright, perfectly round, greyish white spot, with a diameter of 7·2 miles. The smaller crater, however, remains unaltered, and corresponds to Mädler's description. We have here, in fact, an exactly similar case to the well-known instance of Linné. About 240 miles south of this last region, on the border of the grey Mare Nubium, is a fine though ruined walled plain, named *Hesiodus*, and consisting of a level plain nearly 20 miles in diameter, surrounded by the ruins of a mountain border. Beer and Mädler describe and draw this formation as containing a perfectly level interior, grey in colour, and becoming slightly lighter towards the centre. Within it now, nearly in the centre, is a fine deep crater, 4 miles in diameter, and very distinct long after sunrise, so that it seems difficult to understand how it could have escaped Beer and Mädler's notice, had it then existed like it is now. Many other instances of a similar nature might be easily quoted.

Selenographers have also been long acquainted with variations of a different nature, occurring in lunar regions, and consisting in variation in colour or brightness. Beer and

Mädler, who were the first to notice the existence of definite variations in tint, were struck by the analogy they bore to the changes which would result from processes of vegetation on the lunar surface, though, as they pointed out, this seemed incompatible with the absence of masses of water upon the moon. It is remarkable that, despite this apparently fatal objection, other experienced selenographers have been led to the same conclusion, from evidence of widely diverse nature. Other instances of colour changes on the moon, also periodical in character, appear to indicate the effects of the solar rays continuously poured down on the lunar surface for nearly fifteen days at a time; and to this class appears to belong the variation in the floor of Plato, which has been discussed by Mr. Proctor in the "Quarterly Journal of Science" for January and October, 1873. There are other instances which, unlike the last, are not periodical in nature, and which indicate in a very forcible manner the effect of the slow, but energetic and increasing, disintegrative agents which have destroyed all the older lunar formations, and the principal of which appears to be the extensive and all-powerful lunar atmosphere. The capabilities and power of this last member of the forces moulding the surface of the moon are generally overlooked, on the supposition that its small density, when compared with the extremely dense atmosphere of the earth, must destroy its energy and capabilities, forgetting that what it may want in density it may make up in volume.*

The variation in the tint of the floor of Plato is one of the

* It is to be remembered that the influence of the atmosphere upon the surface of the moon depends almost entirely upon its mass, and not upon its surface density. It is not through its pressure that the lunar atmosphere exerts its influence upon the surface of the moon, but through its chemical action upon the constituents of the crust of the moon, or through modifying the great variations in temperature to which the lunar surface is exposed. In its chemical action the effects of the atmosphere will depend almost entirely upon its mass, because, through diffusion and similarly acting causes, all portions of the atmosphere will successively come in contact with the surface. Were the action of the atmosphere very rapid, so that the entire chemical power of any portion was liable to be exhausted before its place could be taken by another portion through the agency of diffusion, then it is true a decrease in the density of the atmosphere might produce a decreased action in a given time. But the action of the atmosphere is so excessively slow that any effect of the kind stated is out of the question. The effect of the atmosphere in modifying the variation in temperature to which the lunar surface is exposed likewise depends almost entirely upon the mass of the atmosphere, and not upon its surface density. Thus, in the retardation of the radiation of heat from the moon, and in the absorption of the solar heat, it is not the surface density of the atmosphere on which these effects depend, but, as both Tyndall and Magnus have shown, upon its mass. Yet the strange idea that the powers of the lunar atmosphere lay entirely in its surface density seems to be entertained by a hostile reviewer of my work on the Moon.

most interesting of these changes in tint, because its reality and its periodical character have been established in an indisputable manner. Plato is a circular plain, 60 miles in diameter, lying depressed beneath the surface of a great belt of highlands, which separate two of the greater lunar dark plains from one another. The formation lying in 50° N. latitude is seen from the earth in perspective, and appears to be foreshortened into a marked ellipse. It is bordered by a moderately steep slope, from 3000 to 3500 feet in height, broken here and there on the west and east by peaks nearly as high again. On the south it is separated by this highland border from the dark plain called the *Mare Imbrium*, to the level of which the border falls in a gentle slope, the distance between the floor of Plato and the Mare Imbrium being about 10 miles. The plain forming the interior of Plato is almost level, being broken only by a few very small steep crater cones. The colour of the highlands around Plato is a pale yellowish grey at sunrise, gradually becoming a greyish white as they are more intensely lit up by the solar rays. The grey plain on the north of Plato, called the Mare Imbrium, is a pure yellow-grey at sunrise, gradually increasing in brightness until a light yellowish grey at full moon. These details are necessary to properly understand the change which occurs in the tint of the floor of Plato as the lunar day progresses from sunrise to noon, and thence to sunset. The details of the changes experienced are founded on six years' observations.

At sunrise the interior of Plato appears a pure cold grey in colour, whilst the surrounding highlands are a yellowish grey, and the neighbouring Mare Imbrium a cold yellow-grey, on the western portion of the floor being the deep black shadow of the border, and on the eastern side the greyish white illuminated slope of a lofty peak. As the sun rises higher above the horizon of Plato, and the solar rays fall more perpendicularly on this region, the whole surface grows rapidly brighter, until, about two days after sunrise, the interior of the formation attains its brightest tint. It is then a cold light yellow-grey, often approaching a pale yellow in fact, and brighter than the surface of the Mare Imbrium on the north, whilst the surrounding highlands are a bright greyish white, tinted here and there with grey. Judging from what occurs in any of the numerous other formations resembling Plato, this may be considered the normal tint, inasmuch as those other formations which present exactly the same phenomena up to this time, and which under similar conditions present exactly the same appear-

ance, retain this tint unaltered until near sunset. After, however, the second day, the floor of Plato commences to undergo a most extraordinary and anomalous change, which renders it unique on the moon, for instead of growing lighter the interior commences to become darker. Four days after sunrise it is materially darker than the northern Mare, and a cold grey in tint, whilst the surrounding highlands are a bright white in colour, tinted with grey; the appearance they retain until the thirteenth day after sunrise growing a little, though not very much, brighter towards full moon. Two days later the floor of Plato has become a dark grey; at full moon it is deep steel-grey; and about two days after full reaches its darkest tint, a very deep steel-grey, almost approaching a black colour. Under these conditions it is one of the very darkest portions of the entire lunar surface, though seven days prior it was one of the lightest portions of the surface of its kind. After this it gradually lightens in tint, but much slower, and never reaches so light a tint.

This extraordinary periodical change in the tint of the floor of Plato has hitherto received no explanation, but its existence has been put beyond the pale of doubt, and Mr. Birt has, at the instance of a British Association Committee, carefully discussed a numerous series of observations, made in the years 1869, 1870, and 1871, by six or seven independent observers. The character of this change, however, has yet to be examined to ascertain whether it is due to a real alteration in the tint of the surface, or is merely an optical variation due to either the physiological effect of contrast or to a peculiar conformation of the surface, or to some cause of similar nature.

It is not impossible to imagine a peculiar conformation of the surface which would cause it to undergo changes somewhat resembling those observed in the case of Plato, and in fact many such analogous explanations might be devised. It, however, must be borne in mind that, though this is possible, it involves what is extremely improbable, because none of these peculiar conformations can be considered in any manner likely. Thus, by supposing the surface of Plato to be covered by a number of conical elevations, each of a particular shape and height, and distributed in a particular manner, and with a systematic relation to every other such conical hill, the broad features of the phenomenon might be accounted for. This is invoking, however, the aid of a most fanciful arrangement, and the probability of any such explanation is too small to be seriously urged for consideration.

Mr. Proctor has advanced, however, a very ingeniously-contrived hypothesis to account for the phenomena observed, and which he regards as being probably the effects of contrast, and this view he has backed by some sound arguments. Mr. Proctor would seek the explanation of the variation in the floor of Plato in the same source as that of the well-known phenomena of a dark body looking still darker when on a brighter background. Thus at sunrise the floor of Plato is thrown against a dark background, due to the sombre, barely illuminated, surrounding regions, whilst at full moon it has for a background the brilliantly illuminated surrounding highlands, and should look much darker. Thus ("Quarterly Journal of Science," vol. xl., p. 504, Oct., 1873) Mr. Proctor says—"Setting aside other possible explanations . . . there is the effect of contrast to be considered. This I believe, from my own observations, to afford the true explanation of the observed phenomena. Plato lies on lunar highlands, which shine very brilliantly under high solar illumination. Towards the Mare Imbrium a comparatively narrow ridge separates the floor from that region. Now when the terminator has just passed beyond Plato, the surrounding wall is not nearly so bright as at the time of full moon; the black shadow of its western ridge occupies the western side of the floor; and the eye, in estimating the tint of Plato, is neither oppressed with the glare of general light on the one hand, nor forced to compare the tint of the floor directly and solely with a much brighter surface; if the comparison is made on the east, with an illuminated wall, it is made on the west with a perfectly black shadow-streak. Similar remarks apply to the time when the terminator is about to pass away from Plato. But at the time of full moon the highlands around Plato are very brilliantly illuminated. The glare necessarily makes Plato itself look relatively dark, notwithstanding the fact that the floor is also much more brilliantly illuminated; for it is a recognised fact, that surfaces of unequal light-reflecting capacity appear to differ more in brightness under a high illumination than when they are only faintly illuminated. We know, in fact, that a surface which is only dark looks almost or perfectly black when itself and a brighter background are under strong illumination." Mr. Proctor proceeds to illustrate this by reference to the fact that occasionally some of Jupiter's satellites look bright when seen against the sky, and yet dark when seen against the bright surface of the planet when in transit, and then continues—"Such an observation as this appears to me to be decisive

against mere eye-estimations, showing that absolutely no reliance can be placed on them unless some contrivance is employed to destroy the effect of contrast. As it is conceded that none of the observed darkenings of the floor of Plato have been other than eye-estimations, no further discussion seems needed, or can in my opinion be legitimately given to the subject. I may mention, however, that having, though as yet in an imperfect manner, studied Plato with a much reduced field, so that I could eliminate to some degree the effects of contrast, I have not found that the floor grows relatively darker towards the time of full moon."

This most ingenious explanation devised by Mr. Proctor to account for the phenomena presented by the floor of Plato, and by assuming the correctness of which he is enabled to draw the very strong conclusions just quoted, is not an explanation which will be for one moment admitted by astronomers who are familiar with the phenomena presented by the various lunar formations, for they at once perceive numerous and apparently fatal objections to it, when applied as Mr. Proctor proposes to do: its very nature, in fact, suggests the idea that its able author overlooked the relationship in which Plato and the anomalous darkening of its floor stood to other similar formations with the normal behaviour of their interiors, for his hypothesis supposes nothing exceptional in the constitution of the walled plain Plato. If, then, the great darkening observed to occur in the tint of the interior of Plato is merely apparent, and only what must occur when a darkish walled plain is surrounded by a bright background, or rather bright environs,—and this is all Mr. Proctor ascribes to it,—it must be a perfectly normal occurrence, and the same must take place in every similarly placed formation, unless that has something anomalous about it to prevent this taking place. But all selenographers could instance a number of such walled plains where no such darkening occurs. Are we, then, to assume that all these possess anomalously constituted interiors, and that only Plato, of all the lunar formations, exhibits the normal phenomena. This is, of course, entirely inadmissible, and selenographers are thoroughly aware that the effects of contrast alluded to by Mr. Proctor are entirely incapable of bringing about such an immense darkening in tint as is apparent in the case of the floor of Plato.

As an instance illustrating this, the behaviour of the walled plain named Archimedes may be considered in connection with that of Plato. This walled plain lies not very far to the south of Plato, and is, if slightly smaller, of the

same form, and bordered by mountains of the same height and brightness; whilst lying on the open plain it is environed by a surface little inferior in brightness to that around Plato. At the period when the anomalous darkening of the floor of Plato commences, the interiors of the two formations are sensibly of the same tint, a cold light yellow grey, both being brighter than the surface of the intervening Mare Imbrium. But while the interior of Plato darkens until it is almost black, the floor of the similar formation Archimedes grows gradually lighter until it is a bright yellowish grey, thus following the same course as nearly every other lunar formation. If, then, the darkening presented by the floor of Plato is, as Mr. Proctor contends, the natural result of the contrast between its surface and its bright environs, why does not Archimedes, similarly placed, and with a floor at one period of similar darkness, present the same phenomena? The fact that Archimedes and the many other similarly placed formations do not present this darkening, and that Plato alone of the many hundred lunar-walled plains does darken to this great extent, shows that there is something anomalous in the constitution of its interior, which is what selenographers contend. It is obvious, therefore, that the hypothesis framed by Mr. Proctor to account for the phenomena fails to do so when critically examined.

In his paper, Mr. Proctor refers, however, to what he considers direct evidence that the darkening of the floor of Plato is a mere effect of contrast, namely, that when examined with a small field of view, so as to shut out the greater portion of the surrounding bright environs, the interior of the walled plain showed no darkening towards full. This observation of Mr. Proctor's, if confirmed, would have been extremely important, because it would establish the fact of the darkening of the interior of Plato being merely the effect of contrast, and so make the general behaviour of the similarly situated walled plains the real anomaly. The observation of Mr. Proctor was confessedly imperfect, but the point required careful examination. For this purpose a numerous series of observations, with the field of view of the telescope purposely constructed to small dimensions, was undertaken, in some the field being reduced so small that only a portion even of the interior of Plato could be seen at once, so that the entire environs were shut out; in others, the border of the walled plain was included; and in others, more or less of the surrounding brighter regions in all less different sized fields were employed, a revolving diaphragm with apertures of different sizes being employed.

With these means a series of several thousand observations, extending over a period of nearly two years, was made, and not only of Plato, but of similarly placed or coloured formations. Nothing new, however, resulted; the interior of Plato went through the same course of darkening when observed through small fields of view as before, and the other regions exhibited their usual course of variations. The observation of Mr. Proctor was not therefore confirmed.

For the purpose of still more decisively settling this point, a piece of apparatus was constructed whereby—through the agency of shifting slides of blackened metal, with different sized apertures—any two or three different small regions of the moon could be seen isolated in the same field, all other portions of the surface being cut out. Moreover, by illuminating the upper side of the dark shutters the great contrast between the bright moon and a dark field could be subdued. In any case, however, as each of the two or three portions were seen at once, under exactly the same conditions, the effect of contrast was removed. By this means, from time to time, the floor of Plato was compared with other small portions of the lunar surface, so that any change would be at once detectable, and a numerous series of observations made. Under these conditions the darkening in the floor of Plato was as marked as ever, though all the effects of contrast with the surrounding bright highlands were entirely removed, for these bright highlands were shut out of view; and as both portions of the moon were thus placed under exactly similar conditions were they of the same brightness they should have so appeared when thus directly compared. By this means the reality of the darkening of the floor of Plato was incontestibly established, and shown to remain unaffected when the effects of contrast are thus eliminated.*

The explanation advanced by Mr. Proctor to account for

* Since this was written a statement has appeared in a letter to an English periodical, from Mr. Proctor, to the effect that, whilst somewhere in America, some American gentleman—neither whose address nor name is mentioned—once told him that he thought no real darkening in the floor of Plato took place. This must be taken *quantum valeat*, and is only mentioned so that all published information on this subject may be referred to. A reviewer of my work on the Moon, in a very curt and *gauche* manner, contradicts the above statement, without giving any grounds for his behaviour. It is to be presumed he drew on himself for the numerous series of observations necessary to authorise a point blank denial of a result established by a long series of elaborate observations made with powerful instruments. Until these are published, however,—and nothing is known of them,—no more need be said of such a contradiction.

the darkening of the floor of Plato is not therefore adequate for its purpose. Unquestionably it may have some effect, but whatever its amount may be it is too small to be sensible. Some small variations in the amount of the darkening of Plato, when observed by the various methods detailed, were also found to exist, but they were relatively very small in amount. These differences were, however, no more than were to be expected.

What, then, is the cause of this singular darkening in the floor of Plato? What reason can exist why that, as the solar altitude increases, the interior of Plato—unlike any other similar formation upon the surface of the moon—should alter in colour and brightness from a cold yellow-grey to a deep steel-grey, or almost black tint? It arises evidently, from what has been stated, from the anomalous conditions prevailing upon the surface of the interior of the wall plain. It is extremely improbable that these anomalous conditions should be an exceptional and fanciful configuration of the surface, and it is more natural to suppose that they exist in reference either to the condition or constitution of the surface. But however successfully the subject may have been pursued so far, here all further progress in elucidating this question is suddenly arrested,—no *matériel* exist for a further investigation. The observations now required are not those of simple observation with the usual astronomical appliances; what is wanted is special observations with special appliances, and these are at present not to be obtained. And, in fact, the great difficulties in the way of making these observations, and the powerful and costly instruments, and the experience required, effectually debar all except a few favoured astronomers from pursuing these observations. Thus, as in so many selenographical problems, patient observation establishes the existence of certain phenomena, but the elucidation of the meaning of the phenomena established is checked for want of special observations that are never made. For these selenographers have to appeal to those astronomers devoted to what has been termed astronomical physics, but they are too much engaged on, to them, more fascinating subjects to be able to assist selenographers.

Although, therefore, the means do not exist for saying, in one manner or another, this change results from this cause or from that cause, it is not fruitless to enquire what their studies have led selenographers to believe to be the cause of this change. The opinion, then, that appears to agree with the views of the most experienced selenographers who have

studied the phenomena presented by Plato, is that the darkening of the floor of Plato results from an actual change due to heating action of the solar rays. Either from the volatilisation of some constituent of the substance composing the interior of Plato, or from some change in its constitution due to the heating effect of the solar rays, the floor darkens in colour. Now, there are more than one substance, known as a constituent of the crust of the earth, that thus darkens when submitted to heat,—some from loss of moisture, others from loss of other substances, and others again apparently from a molecular action. Selenographers cannot say which of these probably forms the floor of Plato, or rather to which of these does the substance probably forming the floor of Plato bear most resemblance; but there is every reason to believe that to one of them the floor of Plato bears a strong analogy.

It has been urged by Mr. Proctor (*loc. cit.*, p. 504) that under these conditions the maximum change on Plato would not occur until long after the sun had attained its meridianal altitude on Plato, whereas observation shows that this takes place shortly after this period. It can be shown that this is not necessarily the case, because the time when the effect of the solar rays reaches its maximum power depends very greatly on the action exerted by the lunar atmosphere. There is not merely a heating action only, but a loss of heat through various causes; so that the problem is complex, and involves the balance of action between the opposite effects. The question of the effect of the lunar atmosphere in modifying the action of the solar rays upon the moon, though one of very great interest, is one involving very great difficulties, more especially as observation and theory appear to be at variance in one or two points. The influence of the lunar atmosphere is, however, sufficiently great to remove the difficulty that has been urged on this point, just in the same manner that the action of the terrestrial atmosphere renders the hottest portion of the terrestrial day close after noon, rather than towards evening. The real power of this active agent, the lunar atmosphere, is rarely properly appreciated, and, in fact, the whole question of the atmospheres of the planets, as if it were forgotten that the whole subject is within certain limits as reducible to the control of the present powers of mathematical analysis as the motions of the planets themselves. In a future number this subject may be returned to, and the real power exerted by a lunar atmosphere on the phenomena presented by the moon, and on the condition of its surface, will be considered.

The instances that have been dealt with in the preceding pages will show that selenographers are not without strong evidence in favour of the opinion that has long been unanimously held by them, that processes of change are still actively at work upon the moon. It must not, however, be supposed that the above are the sole instances which they have recognised, because this is by no means true, only the difficulties in establishing others have led to their omission here. Dealing with a subject such as Selenography, it is only those who are familiar with all its details who properly appreciate the evidence in favour of or against its problems, —just as in the branches of mathematical astronomy, dealing with our satellite, it is only a proficient in mathematical astronomy who could appreciate the difficulties of the lunar theory, and feel a true confidence in the correctness of the manner in which they have been overcome. The difficulties in the way of making the true bearings of selenographical questions properly understood are greater than might be imagined; for even the very elementary fact that volcanic changes such as are now active on the earth would not be recognisable on the moon, in the present state of our acquaintance with the configuration of its surface, is not generally understood. When, however, the attention of astronomers is more generally directed to the study of the lunar surface, Science will be greatly the gainer, as it is there that the past and future of our earth is to be learnt.

II. EVOLUTION BY EXPANSION, VERSUS EVOLUTION BY NATURAL SELECTION.

By J. Huddart.

THERE is nothing more difficult than to disembarrass the mind of a preconceived idea. A belief instilled in childhood and entertained through life, without a shadow of suspicion as to its truth, takes such firm hold of the imagination as positively to unfit the reason to judge impartially of any opinion opposed to that belief, however false and monstrous it may be.

The tradition of the fixity of species has become so deeply rooted in the human mind that only time, overwhelming evidence, and the untiring efforts of such pioneers of thought as Darwin, will ever eradicate the belief in it. He has already combated this belief with such success that he has considerably shaken the opinions on the subject of many eminent naturalists, while he has made more converts to the doctrine of Descent than is generally supposed. He was by no means the first naturalist to discard the old theory of Creation. He, moreover, disclaims to have been the first to have accounted for the gradual development of species by natural selection, stating that in this he was long preceded by Dr. Wells and Mr. Mathews. Owing, however, to the labour and thought he has expended in elaborating the theory, it will doubtless be always chiefly associated with his name. He considers natural selection to have been the main instrument in the formation of species, but allows that sexual selection, climate, geographical distribution, and other minor influences have had their share in the work.

The striking similarity in appearance, habits, and structure presented by many species of plants and animals first caused naturalists to doubt the "Fixity of Species," and to seek a reason for "Unity of Type" as it is called. According to Darwin, "Nothing can be more hopeless than to attempt to explain this similarity of pattern in members of the same class, by utility or the doctrine of final causes. The hopelessness of the attempt has been expressly admitted by Owen in his most interesting work on the Nature of Limbs. On the ordinary view of the independent creation of each being, one can only say that so it is;—that it has pleased the Creator to construct all animals and plants on a uniform plan: but this is not a scientific explanation."

The most obvious way of accounting for the phenomenon is by concluding that species possessing similar characteristics are related to one another. This is the doctrine of descent. We may go further, and presume that families, genera, and classes presenting points of similitude are also related.

But assuming the truth of these conclusions, it is evident that species vary and develop into other forms; also that their development is progressive, as it is well known that each geological era is characterised by higher types of fauna and flora than the preceding one.

There must be a cause for this gradual development of new forms. The agent that effects these changes is, according to the Darwinian theory, natural selection.

The theory of the origin of species by gradual development, or, more properly, by descent, is often regarded as identical with, dependent on, or so closely connected with that of natural selection, as to stand or fall with it. Darwin himself, in many instances, appears to consider development by descent or evolution as almost synonymous with development by natural selection. The theory of natural selection is, however, merely a method of accounting for or explaining the gradual development of species; and it may be sound or not, without affecting the validity of the theory of descent.

It is my object in this paper to compare this theory of natural selection with that which supposes the existence of an innate force,* and to show that the latter affords the more satisfactory explanation of the principal facts of natural history, physiology, palæontology, &c.

For this purpose I will assume the truth of the theory of the origin of species by gradual development. It has been so ably defended by Darwin, in his "Origin of Species," as to leave no new argument to be urged in its favour. The greatest difficulty which it presents, and perhaps the only one of any importance, is the absence of intermediate links or gradations between existing species: this he accounts for on the theory of the survival of the fittest only in the struggle for life, and by the imperfection of the geological records.

The inadequacy of the theory of natural selection to explain all the phenomena of natural history in a thoroughly satisfactory manner has undoubtedly caused many eminent naturalists to retain their belief in the old theory of creation.

Notwithstanding all the ingenuity of Darwin's arguments, and the vast amount of learning and scientific research he has brought to bear on the subject in support of his theory, a thoughtful reader—though much interested, and perhaps somewhat perplexed—must close his work on the "Origin of Species" with a feeling of dissatisfaction; he cannot realise that such haphazard means as natural selection and the survival of the fittest can alone have wrought such marvels as are exhibited throughout creation; nor, on the *primâ facie* view of the case, that they alone

* The term "Force," it must be clearly understood, is used in this paper, not in the sense of "Germ Force" or "Will Force," but to express merely a faculty of development; and that the word "growth" is used, not in the restricted sense of mere "enlargement," but in the more extended one of "progress" or "improvement."

can have built up an ever-progressing fauna and flora, infinite in variety, but always subject to definite laws, always retaining their typical characters, while evincing adaptation the most perfect to the conditions of existence.

It is not my intention to review the numberless objections to the theory of natural selection that have been urged by different naturalists, prominent among whom are Mivart, Nägeli, &c.; but to show how completely and fully the theory of an innate power of growth or expansion is borne out by the most important phenomena of morphology, and embryology taken in connection with palæontology.

In advocating the theory of expansion, I am prepared to admit that natural selection has probably been a subsidiary agent, and one of some importance; but the agency I believe to have mainly effected what Darwin ascribes to natural selection would seem to be a subtile force inherent in all organisms, too potent to be disturbed in its action to any considerable extent by extraneous influences, and destined to work certain definite results in a definite period of time; in short, a power of growth or expansion implanted in all organisms, when launched into existence, and extending through all time. This power of growth would also appear to develop in all organisms similar features, subject to the modifying effects of climate, conditions of life, &c., at the same stage of expansion.

I will consider this definition of the Innate Force in reference, first, to the phenomena of Morphology.

All plants and animals are classed according to their morphological characteristics, and not with reference to their habits, &c. The habits of different species belonging to the same class are often as different as it is possible to conceive, and they use homologous parts,—that is, corresponding portions of their structures made on the same model or type for widely different purposes. That is to say, species preserve their type no matter what be their conditions of existence. Conformity to type is thus the paramount law in the formation of species; adaptation to the conditions of existence a secondary law. Darwin considers the latter law to be the higher: were this the case we should not find the former asserting itself with unvarying persistence in the most diverse adaptations of homologous parts. Type is persistent through every adaptation. Adaptation is nothing more than a modification of type to suit certain conditions. Adaptation may be the result of natural selection, or it may not; but type cannot be, as its existence is incontestibly independent of, and has no connection with,

the conditions of existence, which, however, wholly influence natural selection. In illustration of this I cannot do better than quote Darwin. When writing on the subject of morphology, he says—"This is one of the most interesting departments of natural history, and may be said to be its very soul. What can be more curious than that the hand of a man, formed for grasping, that of a mole for digging, the leg of a horse, the paddle of a porpoise, and the wing of a bat, should all be constructed on the same pattern, and should include similar bones, in the same relative positions? How curious it is, to give a subordinate though striking instance, that the hind feet of the kangaroo, which are so well fitted for bounding over the open plains,—those of the climbing, leaf-eating koala equally well fitted for grasping the branches of trees,—those of the ground-dwelling, insect- or root-eating bandicoots,—and those of some other Australian marsupials,—should all be constructed on the same extraordinary type, namely, with the bones of the second and third digits extremely slender and enveloped within the same skin, so that they appear like a single toe furnished with two claws. Notwithstanding this similarity of pattern, it is obvious that the hind feet of these several animals are used for as widely different purposes as it is possible to conceive. The case is rendered all the more striking by the American opossums, which follow nearly the same habits of life as some of their Australian relatives, having feet constructed on the ordinary plan. Prof. Flower, from whom these statements are taken, remarks in conclusion—'We may call this conformity to type, without getting much nearer to an explanation of the phenomenon;' and then he adds—'but is it not powerfully suggestive of true relationship, of inheritance from a common ancestor?'"

It is certainly suggestive of this, and of much more. It seems to show very clearly that morphological characters, referable to type only, do not vary with the conditions of existence.

A very remarkable instance of adaptation to the conditions of existence is exhibited by many species of flat-fish, which swim with the median plane horizontal, instead of vertical as in other fishes, and which, while maintaining their type, have heads actually distorted in a very singular manner to suit their habits.

Were natural selection the sole architect of animal life, it would be expected that members fulfilling similar offices would be similarly constructed, instead of conforming to the inexorable law of unity of type. But we find that any part of

an organism, belonging to no matter what class, is capable of adaptation to any use, within certain limits, to suit the conditions of existence of its possessor; moreover, that homologous parts have often totally dissimilar functions in different species; for instance, the skull is supposed to be composed of metamorphosed vertebræ, and crustaceans having complex mouths have few legs, while those with simpler mouths have almost invariably more numerous legs; thus, even organs of locomotion may be converted into jaws. There is no reason, deducible from the theory of natural selection, why, in acquiring a more complex mouth, a crustacean should be deprived of some of its legs. It is simply a consequence of the laws of correlation of growth, and would seem to indicate that at a certain period of development the different systems of an animal are capable of modification, but not of extension.

This phenomenon of correlative growths is merely another form of conformity to type.

Darwin himself seems to perceive, in this correlation of growth, the presence of a higher power than mere natural selection. On the subject of domestication he says—"Hairless dogs have imperfect teeth; long-haired and coarse-haired animals are apt, as is asserted, to have long or many horns; pigeons with feathered feet have skin between their outer toes, pigeons with short beaks have small feet, and those with long beaks large feet. Hence if a man goes on selecting, and thus augmenting any peculiarity, he will certainly modify unintentionally other parts of the structure, owing to the *mysterious laws of correlation*."

What is this mystery but the existence of an innate force? What are these laws of correlation but the evidences of its existence?

The existence of rudimentary parts is essentially a conformation to type, and another phase of correlation of growth. It perhaps shows, more clearly than any facts yet mentioned, the nature of the principle of expansion. Rudimentary parts should be regarded not as organs or members in a state of transition, but as developments correlative to those of certain other portions of the organism that are in use; for it can abundantly be shown that rudiments of all kinds exist in almost every organic structure that not only are useless to the individual, but that never can have been, nor ever can be, of any use to the race. The only way of explaining the fact of their existence is by the supposition that organisms do not change their characteristics one by one, but all simultaneously by a slow process of growth, so that no one part

can be behind another in its stage of development. Thus the dog-fish, skate, and other fishes of the kind have partially lost most of their fish-characteristics together. The bristles of the dog-fish and other cartilaginous fishes may be regarded as rudimentary fur, such as the seal possesses. Most apes have simultaneously lost the callosities, the cheek-pouches, the tail, the pointed ears, &c., peculiar to the monkey proper. The swinging of a man's arms in unison with the movements of his legs in walking has reference to correlative development.

Darwin regards rudimentary parts that are apparently disappearing as in a state of decadence through disuse; he fails, however, satisfactorily to explain the existence of those in a nascent state, but of no present use; still less of those in an apparently permanent condition—such as the mammæ of males. It is difficult to imagine that the sexes of Mammalia were ever so confused as to give rise to this phenomenon, especially when the sexes of their immediate progenitors are so perfectly defined.

There is another difficulty, with regard to organs in this state, that Darwin freely confesses has fairly baffled him; natural selection alone cannot account for it. He admits that some additional explanation, which he cannot give, is necessary to explain the still further reduction in size or complete obliteration of an organ that appears to have reached the limit of decadence from disuse, and that it is scarcely possible that disuse can go on producing any further effect after the organ has once been rendered functionless. The explanation of this case, as well as of all other cases of rudimentary growths, is obvious enough on the theory of correlative expansion. Thus it would appear that all parts of an organism and its characteristic features are functions of one another and of the whole organism, and all vary together. The symmetry of form, so noticeable throughout animate nature, is another result of the laws of correlation. Were natural selection permitted to mould the forms of life around us, uncontrolled and undirected by a supreme force, that of correlative expansion, shapes the most grotesque and monstrous would inevitably inhabit the globe.

Instances showing that type is independent of the conditions of existence, that rudimentary parts can be satisfactorily accounted for only by regarding their existence as due to correlation of growth or expansion, and that correlative expansion is a phase of the phenomenon of conformity to type, might be multiplied to any extent.

From these considerations, while agreeing with Darwin that community of type indicates descent from a common ancestor, *I consider that the type was not acquired by that ancestor, but was inherent in it, and still is inherent in its progeny.*

Type itself undoubtedly also develops and changes, but without reference to the conditions of existence or extraneous influences, and solely by expansion through age after age, expansion wrought by the mysterious inherent force performing its work in a set time with the exactness and regularity observable in all operations of Nature.

Modification of type to suit the conditions of existence must be more variable than type itself, as the same typical characteristics are common to a vast number of species adapted to most diverse conditions of life. There are comparatively few leading types, but an enormous variety of adaptations of each type. The type of an organism therefore changes very slowly in comparison to the alterations in its structure due to extraneous causes.

These conclusions, arrived at from the evidence of morphological phenomena, are borne out in a very striking manner by those of embryology, taken in connection with the facts of palæontology.

A study of these subjects also reveals to us more fully the nature of an organism.

The embryos or larvæ of all animals pass through many phases in the course of development. At certain stages the embryos of reptiles, birds, and mammals so closely resemble one another as to be undistinguishable. The embryo of the lower organisation is, in this class of animals, arrested in its growth, or more properly development, at an earlier stage than that of the higher: that is to say, the unformed embryonic mass does not at once acquire the characteristics of its species, but first assumes the form of a remote progenitor of its class, and expands through progressive stages till it arrives at the degree of development of its parents. In these stages the embryo frequently resembles extinct members of the class to which it belongs. In short, the embryo is developed by the same stages by which the race to which it belongs was developed in the course of successive geological eras; *i.e.*, the development-history of the individual is a recapitulation of the development-history of the stock to which the individual belongs. The various phases are so merged into one another, in the development or expansion during gestation, as somewhat to obliterate their characteristic traits and to render them indefinite, but still they indubitably accurately correspond to the stages of

descent. Thus an organism is expanded in the short period of gestation or incubation in precisely the same manner as its progenitors have been expanded through thousands of generations.

What is the conclusion to be formed from these marvellous facts? That the organism is descended from ancestors possessing the characters presented by the embryo? Certainly this, and much more,—namely, *that there indubitably exists in all organisms an innate power of expansion by which at least the individual is developed*, for it is impossible that the egg (of a bird, for instance, during the period of incubation) can be acted on by extraneous modifying influences; and it is a fair and logical inference that the agent that produced the metamorphoses in the development of the race is identical with the agent which produces precisely similar metamorphoses in the development of the individual. What can be more easy to conceive than that each individual may be expanded in an extremely slight degree beyond the stage of development of its parents?

What has been remarked with respect to the embryonic development of the higher forms of life is equally applicable to the analogous phenomena of the metamorphoses of insects. The larval development is an expansion to a state of higher organisation, and takes place generally in the pupa, where no influence but an innate power can effect the changes.

We may not be able to trace descent in the embryo beyond a certain point, and some stages are in many species suppressed that are exhibited by others. But, reasoning by analogy and by the evidence of geological records, we may conclude that the progenitors of all organisms must be among the earliest, and therefore lowest, forms of life. Thus it would appear that the simplest known forms of life are endowed, equally with the unformed embryo or seed, with a power of growth or expansion, a faculty of being drawn out through certain stages which occur always in the same order and with the same correlative features. The germs of each and every system that make up the entire structure, of a bird for instance, exist as certainly in the lancelet (*Amphioxus*, lowest of the vertebrata) as in the egg of the bird at the commencement of the period of incubation; they are, so to speak, latent in each case. In the first case, the development of those germs will occupy millions of ages; in the second case, such age-development, having already been consummated, is recapitulated in the course of a few days.

I have stated that larvæ in their development ascend in the scale of organisation. Darwin says that some descend. If the fact of the descent be real, and not only apparent, it presents a very formidable difficulty. But, as Darwin also remarks, it is very difficult to determine what constitutes the higher organism. It certainly is not superiority in any particular quality, or greater perfection of any one organ. He says that the embryo in course of development generally rises in organisation, and that he uses that expression though he is aware that it is hardly possible to define clearly what is meant by the organisation being higher or lower. Considering that development is almost, if not quite, universally progressive, and not retrogressive,—in short, that progress is the established law of Nature,—the higher organism may, I think, safely be defined to be that which has been produced in the later stage of evolution.

Characteristics acquired by natural selection alone would supersede, permanently and entirely, earlier adaptations, as there is no reason deducible from the supposition that the formation of species is wholly due to the conditions of existence, to account for organisms reverting to ancestral types in their embryos or larvæ. Peculiarities of structure, the result of extraneous influences, when once lost or replaced by others, could not recur either in the embryonic state or at maturity without the exciting cause of the same extraneous influences. Ancestral type alone is reproduced in the embryo, and not the modification of type due to natural selection,—the grand leading characters, but not the innumerable adaptations.

Thus the phenomena of embryology, while strongly supporting the theory of descent with modification, decidedly militate against the supposition that all structural changes are due to natural selection.

Darwin, in quoting Mr. Lewes, remarks that the tadpole of the common salamander or water-newt "has gills, and passes its existence in the water; but the *Salamandra atra*, which lives high up among the mountains, brings forth its young full-formed. This animal never lives in the water. Yet if we open a gravid female, we find tadpoles inside her with exquisitely feathered gills; and when placed in water they swim about like the tadpoles of the common water-newt. Obviously this aquatic organisation has no reference to the future life of the animal, nor has it any adaptation to its embryonic condition; it has solely reference to ancestral adaptations,—*it repeats a phase in the development of its progenitors.*"

This is a case very much to the point. We cannot conceive a recurrence to ancestral adaptation due to natural selection without a distinctly beneficial result accruing from it, or without some direct cause for it. The recurrence to ancestral forms can only be a recapitulation of earlier types.

Having thus briefly reviewed the evidence of the leading facts of natural history and allied subjects, in reference to my theory, I will now arrange what deductions I have made from them, in such a form as to afford a clear appreciation of my position.

First, I have shown that type is persistent through every adaptation to the conditions of existence ; or, in other words, that adaptation to the conditions of existence is independent of type. From this I infer that *conformity to type is due to a cause separate and distinct from that which effects adaptation to the conditions of existence ;* the latter may therefore be the result of natural selection, but the former must be due to an innate power,—that is, natural selection may modify, but cannot originate type.

Secondly, I have pointed out that a power of expansion indubitably exists in all organisms,—that it produces in the individual a repetition of the metamorphoses that have occurred in the race. From this similarity of effect I infer a similarity of cause ; *i.e., that the race was developed by a process of expansion, or unfolding.*

I have, moreover, shown that correlation of growth can be satisfactorily explained only on the supposition of correlative expansion. I have also indicated the impossibility of explaining the recurrence of organisms to ancestral types in their embryonic or larval stages, on the theory of natural selection.

I may here mention how strikingly the theory of an innate force harmonises with the well-known fact that similar forms of life have appeared simultaneously all over the world. This is accounted for by Darwin on the supposition that dominant species have a tendency to spread widely ; but this does not account for the distribution of all species with sufficient rapidity to represent this phenomenon ; for instance, terrestrial Mammalia would not migrate to any great extent, and very slight barriers would confine them to a limited area for generations.

The phenomenon is, however, decidedly suggestive of the gradual evolution of species by some agency acting at a uniform rate throughout the world.

The appearance at the present time, in similar climates, of plants and animals closely resembling one another, and

yet distinctly different in continents or tracts separated by natural barriers, would indicate not so much relationship as expansion from a similar source by the same laws, the slight divergence of character being due to a difference in the conditions of existence.

I have not yet touched on the subjects of instinct, organs of high perfection, and the evidences of design : they do not appear to me to admit of the exact reasoning applicable to, and the strictly logical conclusions deducible from, the leading facts—the grand truths of natural history on which I have in this paper based my arguments in support of the theory of evolution by expansion.

Darwin is well aware of the difficulties they present to the theory of natural selection, and he has met these difficulties by arguments more admirable for their ingenuity than for their conclusiveness. Thus, he says of instinct,—"Many instincts are so wonderful that their development will probably appear to the reader a difficulty sufficient to overthrow my whole theory. I may here premise that I have nothing to do with the origin of the mental powers, any more than I have with that of life itself. We are concerned only with the diversities of instinct and of the other mental faculties in animals of the same class."

He here avoids raising a query that can, it seems to me, admit of only one answer.

The same difficulty occurs to him in connection with organs of extreme perfection, and is disposed of in the same way. Thus he says—"To suppose that the eye, with all its admirable contrivances for adjusting the focus to different distances, for admitting different amounts of light, and for the correction of spherical and chromatic aberration, could have been formed by natural selection, seems, I freely confess, absurd in the highest degree. When it was first said that the sun stood still and the world turned round, the common sense of mankind declared the doctrine false, but the old saying of *Vox populi vox Dei*, as every philosopher knows, cannot be trusted in Science. Reason tells me that if numerous gradations from a simple and imperfect eye to one complex and perfect can be shown to exist, each grade being useful to its possessor, as is certainly the case ; if, further, the eye ever varies and the variations are inherited, as is certainly likewise the case ; and if such variations should be useful, under changing conditions of life, then the difficulty of believing that a perfect and complex eye could be formed by natural selection, though insuperable by our imagination, should not be considered subversive of the

theory. How a nerve comes to be sensitive to light hardly concerns us more than how life itself originated."

Not common sense only, but Reason rebels, when required to acquiesce in a theory which supposes that an eye, perfect and elaborately constructed in accordance with occult laws of physics, could have been formed by a series of accidental variations—as it would if required to believe that a philosophical instrument made by man could have been evolved by such means.

But there is no difficulty in allowing that the most perfect eye has been gradually developed from the most imperfect, provided we recognise the existence of an agent adequate to the accomplishment or achievement of such development.

The existence of the evidences of design in creation is so much a matter of opinion that it would serve no purpose to discuss it here; if, however, it be once admitted that design is apparent in the different forms of life, the theory that natural selection has been the chief agent in their formation becomes untenable.

It is needless to point out how the above difficulties to the theory of natural selection tend to corroborate the theory advocated in this paper.

When Darwin evades more searching investigation of his difficulties by coming to the conclusion that to attempt to explain them more fully than he does would involve an inquiry into the origin of life itself: when he uses such expressions as "the mysterious laws of correlation;" when, indeed, he assumes that an organism has the faculty of adapting itself to its condition of life, has the power of improving its members or organs, and altering them to serve for new purposes; when he absolutely founds his theory on the supposition that an organism is endowed with the power, by a long series of efforts, and the inheritance of slight variations, of developing into a new and more highly organised structure, does he not tacitly admit the existence of an inherent force or energy, or, in other words, of a power of expansion? External conditions would have no more effect on organic than on inorganic matter, were not the former endowed with a power of growth, development, expansion. Were organisms merely plastic there could be no progress, no improvement, no ascent to higher forms of life; there might be variety or change, but there would be nothing new or essentially different. "Ex nihilo, nihil fit;" in default of a power of expansion extraneous influences would have nothing to work upon, no energy to direct, no growth to mould to the changing conditions of life.

I have seen it stated that the theory of natural selection does not suppose the individual to be capable of self-improvement, but that purely fortuitous variations are rendered permanent by inheritance provided they happen to be improvements, and that thus only have the different forms of life been produced. But Darwin certainly believes that some variations are due to effort, or use and disuse, caused, however, entirely by the accidental circumstances of the conditions of life, and that variations that are inherited become characteristics of new species.* Although he has not expressly excluded the possibility of the action of some other agency, the general tenour of his writings is sufficient to convince us that in his opinion natural selection is sufficient to account for the origin of species. The only instances in which he appears to entertain misgivings on this point have been already mentioned.

The chief difficulty of the theory of expansion is the fact of the existence at the present time of the lowest forms of life. Several explanations suggest themselves:—

First. The germs of life may exist inactive for many ages, until activity is set up by some particular cause.
Secondly. The germs of life may be capable of spontaneous generation.
Thirdly. Certain germs may have a limited power of development.
Fourthly. Certain germs may be retarded or completely arrested in their development from some cause or other.

These suggestions are purely speculative; they are given merely to show that the difficulty admits of explanation in various ways.

I have chosen the word "expansion" as the one open to the fewest objections when used in its mathematical sense. The word "growth" would confuse the age-development with the growth towards maturity of the individual, from which it is quite distinct. The words "development" and "evolution" express only improvement or extension, but "expansion" conveys the idea of development by the action of some force or agent, or else extension according to a definite law, such as the expansion of a binomial into an infinite series.†

* See Preface to second edition of "Descent of Man."

† The word "Expansion" has been chosen, also, to distinguish the theory from that of "Progressive Development" advanced by Lamarck, and which he admits to be merely a statement of observed facts, and from those that suppose the existence of an innate tendency to improvement and perfection; in short, to distinguish a process of "unfolding" from one of "addition."

In conclusion, I must remark that it appears to me to be more philosophical, and more in accordance with what we know of the operations of Nature, to believe that species were formed by rigid and unchanging laws, acting through all time, than to suppose them to have been evolved by a series of accidents.

It is, indeed, a tremendous thought, that the highest forms of life have been expanding to their present state of perfection through hundreds of ages; that all forms of life are now unfolding and expanding to higher types,—expanding not by uncertain and fortuitous means, but by laws as immutable and inexorable as those which govern inanimate matter; that the great scheme of evolution had been fully planned and matured when the first minute germs of life, with their mighty destinies, came into active existence in a world which was then a wilderness, but which has since become full of glorious life, of forms most wonderful and beautiful, evolved by the mysterious power acting with ceaseless energy through all the bygone ages.

The imagination is dazzled in the contemplation of results so stupendous from a beginning apparently so insignificant; but the reason must needs bow before the great weight of evidence, and must admit the inevitable conclusion that in every living thing there is a force that for ever works upwards and onwards, that retrogression and decadence are impossible, save for a transient wave of apparent degeneration that seems occasionally to sweep over a race, and which is generally attributable to a temporary extraneous cause.

It would seem that Tennyson, with the divine inspiration of the poet, had some dim conception of the great truth when he wrote—

> "A monstrous eft was of old the Lord and Master of Earth,
> For him did his high sun flame, and his river billowing ran,
> And he felt himself in his force to be Nature's crowning race.
> *As nine months go to the shaping an infant ripe for his birth*
> *So many a million of ages have gone to the making of man:*
> He now is first, but is he the last? is he not too base?"

III. THE PORT OF YMUIDEN.

By F. C. DANVERS, Assoc. Inst. C.E.

FEW people will recognise in the above name the great and important engineering work which has, since 1865, been under construction in Holland, and which has been more familiarly known as the "Amsterdam" or "North Sea Canal." Since the construction of the Suez Canal no hydraulic work of greater, or even equal, importance has been undertaken, and its successful completion reflects no little credit upon Sir John Hawkshaw and the able engineers who have been associated with him in the undertaking. A brief retrospect will show the necessity that existed for this work.

In the sixteenth century Amsterdam stood first of all the commercial cities of Europe. Its prosperity, however, gradually began to decline, partly from the rise of other ports, but principally from the difficulties of navigation caused by the silting up of the Zuyder Zee, and above all by the formation of the Pampas Bar. Large vessels could, in consequence, no longer get up to Amsterdam with their cargoes, but were obliged to discharge outside the bar, when they were floated over it by means of "camels," which, when the water was pumped out of them, raised the vessel with them. The trade of Amsterdam was fast being diverted to Rotterdam, but in 1819 steps were taken to avoid the difficulties of the Zuyder Zee by the construction of the North Holland Canal, which extends from Buiksluyt, opposite Amsterdam, to the Helder, a distance of 51 miles; and this canal has since been extensively used by vessels of large burden seeking the North Sea. It is, however, difficult of passage in winter, and consequently it was at length determined to connect Amsterdam with the North Sea direct by means of a canal passing through the Wijker-meer and the Ij, by the shortest possible route, by the adoption of which the distance to the sea is shortened by no less than 36 miles.

This work was commenced in March, 1865. It consists of two parts, viz., a harbour in the North Sea, at the mouth of the canal, and the canal itself.

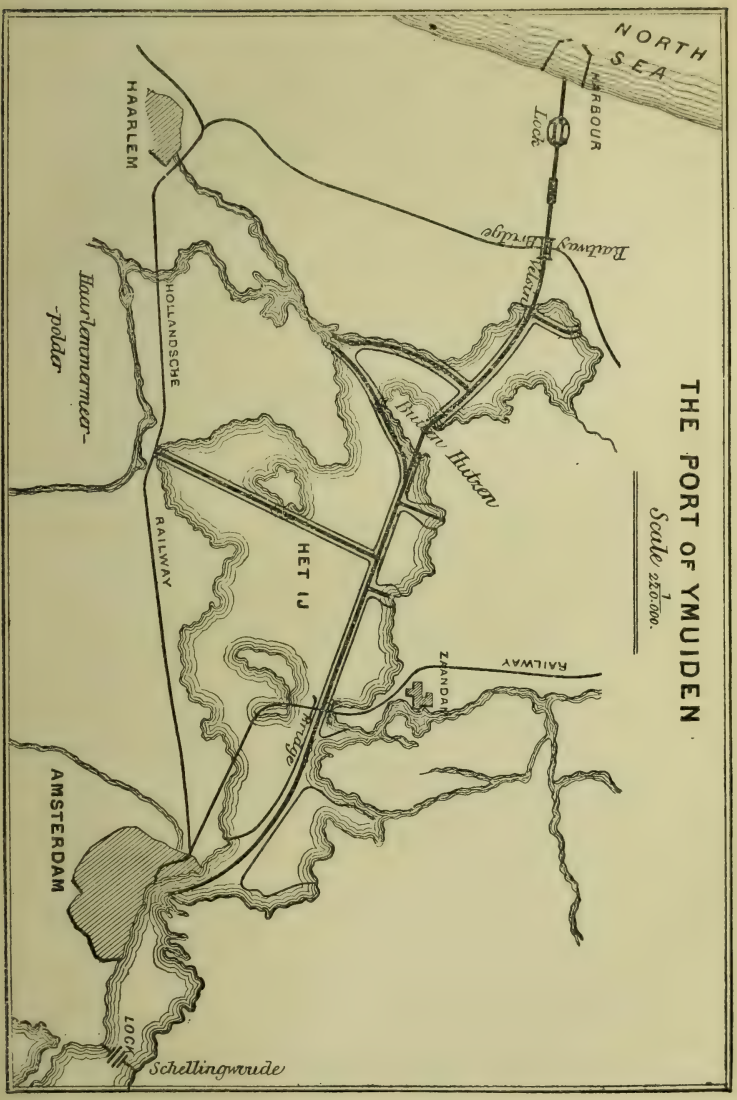
The harbour was designed by Sir John Hawkshaw. It consists of two piers stretching out into the sea to the depth of about 26 feet below the level of low water. These piers at their junction with the land, at the foot of the downs on the

beach, are 1312 yards distant from one another. Thence their directions converge, so as to make an angle of 77 degrees with their base line, and at 1312 yards from the shore they are 721¾ yards apart. From this point they begin to converge more rapidly, so that, with an additional length of 377 yards,—making 1689 yards in total length, —they terminate 284 yards apart at the harbour mouth.

The piers consist of a solid sea wall, 27 feet wide at top, and increasing in thickness downwards by one-seventh of their height. They are formed as follows:—First of all a foundation of basalt rock was thrown into the sea, about 40 yards wide and 1 yard in thickness. On this a superstructure is raised, formed of concrete blocks, varying from 4 to 10 tons in weight. The rubble foundation was levelled by divers, by whom also the blocks below low water were fitted, without any cement in their joints, the blocks being let down to their site by a powerful steam crane called a "Titan." From low water the blocks are laid in cement; for the lowest joints—which are only a few hours above water—a quick-setting Medina cement was used, and for the higher joints Portland cement. The top of the piers is generally 13 feet 6 inches above the Amsterdam pile (datum). The parapet on the top of each pier consists entirely of concrete, moulded on the spot in frames erected *in situ*. Behind this is a pavement of brick in cement, supported by a band of granite along the inner top line. In the construction of each of the two piers there has been used 433,600 yards of concrete blocks, 77,700 tons of cement, 229,000 cubic yards of shingle, 130,900 cubic yards of basalt, and 19,035 cubic yards of broken bricks, the last-named being used in the formation of the concrete blocks placed in the middle of each pier.

In order to afford further protection to the piers against the force of the waves, a slope of rubble rock has been thrown into the sea on the outside of each pier, the lower blocks of these wave-breakers being 10 tons each in weight, and those above low water about 20 tons each. The harbour will afford berths for three hundred large vessels.

The canal, in passing from the harbour through the downs, runs eastward at first in the direction of an arc of a circle, with the concave side turned southwards, for a distance of 1300 yards, at the end of which are the North Sea locks. The curved form of the canal in this portion serves principally to protect the locks from the force of the waves. From this point the canal runs for some distance in a straight line, and afterwards sometimes in curves, with occasional straight reaches, to Amsterdam—a total distance of 25,919 yards.


THE PORT OF YMUIDEN
Scale 1/220.000.
NORTH SEA
HARBOUR
Lock
Railway Bridge
Velsen
Buiten Huizen
HAARLEM
HOLLANDSCHE
Haarlemmermeer-polder
RAILWAY
HET IJ
ZAANDAM
RAILWAY
Bridge
AMSTERDAM
LOCK
Schellingwoude

Two sections of the canal pass through dry land; the one from the North Sea to Wijkermeer, at Valsen, 6540 yards long; and the other across the peninsula of Beutzenhuisen, 875 yards in length. The portion of the canal passing through the Wijkermeer has a length of 4779 yards, and that through the Western Ij one of 13,725 yards.

The canal is formed with a bottom width of 88 feet 6 ins., and a surface width of 207 feet, and when finally completed it will be over 23 feet in depth. These dimensions continue throughout 22,966 yards in length of the canal, measured from the North Sea, after which the width gradually increases until it joins the "diep" near Amsterdam: here the dykes turn away from the canal, the northern one running in a straight line to Buikslooterham, and the southerly one in a curved line to Amsterdam.

The earth excavated from the land sections of the canal is conveyed in barges, and deposited on the muddy bottom of the lake to form dykes on either side, where it passes through the Wijkermeer and the Ij. These deposits are then covered with clay, and as soon as the bank appears above the surface of the water, and is formed, the slopes are protected from the wash of the waves with fascines. The canal channel is then dredged between the banks so formed, for which purpose steam dredgers are employed with self-acting discharging apparatus. This apparatus consists of a vertical cylinder fitted on to the steam dredger, into which the excavated mud is thrown. On the lower end of this cylinder a horizontal centrifugal pump-wheel, of about 40 ins. in diameter, works, which is driven by the engine of the steam dredger, at a speed of 230 revolutions a minute, forcing the mud from the cylinder, in a semi-fluid state, into a floating tube, through which it is carried away. The floating tubes consist of wooden cylinders of about 50 feet in length and 15 inches in diameter, joined together by flexible leather couplings. These float on the surface of the water for distances of from 750 to 800 feet, the ends being carried through and sometimes over the previously-made sandy dykes, behind which a jet of muddy water is discharged, containing from 40 to 50 per cent of solid matter, which thus becomes deposited over a considerable surface. From 1200 to 1500 tons of sand is raised by one dredger, with a 20-horse power engine, per day.

In order to connect the different navigation and drainage locks and sluices along the borders of the Ij with the main canal, several branch canals have been constructed, of varying sections, and having an aggregate length about

equal to that of the main canal. The total length of dykes constructed is about 40 miles, and the excavation from the canals has amounted to about 13 millions of cubic yards of sand and mud.

The spaces outside the canal, being pumped dry, become suitable for meadows, or, as they are called "polders." In order to keep them dry from percolation or drainage, pumping-engines are placed at certain distances along the canal, which drain them and empty the water into the canal. The total amount reclaimed, and to be reclaimed, is 12,450 acres. These polders continue along the canal side until the sand dunes are reached at 3 miles from the west coast.

The canal is crossed at two points by railway bridges, but the most important engineering works are the locks at either end of the canal, which deserve a brief description here.

As has been already stated, the North Sea locks are placed 1300 yards from the western end of the canal. In these, special precautions were necessary to protect them from high tides. The locks are unusually high and strong, and are provided with extra gates, to be used when the tide rises above a certain height. Here there are two locks and a sluice, the larger of the two locks being capable of admitting a vessel 393½ feet long and 59 feet wide, with a depth over sill of 28 feet 8 inches at ordinary low water. The walls of this lock are of brickwork, the exposed faces of which are of pressed bricks, and the rest of the work of ordinary hard burned bricks. In the case of the smaller lock an earthen slope supplies the place of the southern wall along the lock pond.

The Zuyder Zee locks at Schellingwoude have been named the "Orange Locks," after the heir apparent to the throne of Holland. Side by side in the long dyke, which is nearly a mile long, are three locks for ships, one sluicing lock, and three masonry channels for pumping out, by steam, the water when the tide in the Zuyder Zee is higher than the water in the canal. The largest ship lock is 315 feet long and 59 feet wide; the length of the other two is 239 feet, and their width 46 feet. These locks are built on a foundation of 8896 piles, and the dyke rests on mattrasses of fascines laid on the mud. The dyke is composed of sand and clay laid on fascines, and faced on the sea side with blocks of basalt and granite covered with clay. These locks have altogether twenty-seven pairs of gates, of which eleven pairs are of iron and sixteen of wood. The lock

walls are built exclusively of ordinary brickwork, in the same manner as those of the North Sea locks.

It will thus be seen that, although the canal itself is shorter, the works on it are considerably more important than anything that occurs on the Suez Canal. The "Orange Locks" were opened by the King of Holland in 1872, and the completed canal was opened by His Majesty on the 1st of November last, when he ordered that it should be known by the name of "The Port of Ymuiden." The total cost of this stupendous work has been about two millions sterling.

IV. ANIMAL GEOGRAPHY.*

ONE of the most striking distinctions between the old and the new school of Natural History is the greatly increased amount of attention paid in the present day to the *locality* of every species. Our predecessors, if any specimen had not been derived from their own country, quietly dubbed it "exotic." Any creature from a tropical climate was labelled as a native of "the Indies,"—which might include either Venezuela, Hindustan, or New Guinea. To the modern naturalist, on the contrary, an accurate knowledge of the locality of every specimen he examines is a point of the first moment. Without this he regards it in much the same manner as a lawyer looks upon an unsigned document. "The structure, affinities, and habits of a species now form only a part of its natural history. We require also to know its exact range at the present day and in pre-historic times, and to have some knowledge of its geological age, the place of its first appearance upon the globe, and of the various extinct forms most nearly allied to it."

But though the correct locality of each species is now recorded in every systematic work on natural history, though local faunæ have been compiled, and attempts made at a general classification of the animal world from a geographical point of view, a work was still wanting which should com-

* The Geographical Distribution of Animals, with a Study of the Relations of Living and Extinct Faunas as elucidating the Past Changes of the Earth's Surface. By ALFRED RUSSEL WALLACE. London: Macmillan and Co. Opening Address of the Biological Section of the British Association, 1876. By the President, ALFRED RUSSEL WALLACE.

bine and harmonise the mass of unconnected facts ascertained, and which should not merely propose an arrangement, but should demonstrate it by a careful and exhaustive analysis. This deficiency has been supplied by Mr. Wallace in a manner which must greatly enhance the well-merited esteem in which he is held by naturalists. The result is a work which in its department has no equal in any language, and which must at once be received as the text-book of zoological geography.

It may, at the first glance, appear an easy matter to determine the geographical distribution of the animal kingdom. We have only, it is said, to take a census of species in every country, to compare the returns, and to arrange our divisions accordingly; but the moment we make the attempt difficulties spring up on all sides. We require a trustworthy classification of animals, so that we may know what forms can be legitimately included under each species, genus, or family. We must then decide whether our classification is to be positive or negative, founded on the presence or on the mere absence of certain groups. Our own view, like that of Mr. Wallace, is that mere negative characteristics can have but very limited value. The extirpation of certain striking forms of life in a given island, whether effected by human agency or by natural causes, cannot give such island a higher rank as a zoological province than it had before. To distinguish two regions, *a* and *b*, we must be able to show that each contains something which is wanting in the other. Why, for instance, are the claims of Australia to rank as a distinct primary region so universally allowed? Not from the mere absence of monodelphic mammals, whether Carnivora, Rodentia, Ungulata, and the like, but because, in the stead of all these, there are didelphic groups which to some extent replace, or at least simulate, the monodelphic orders and families. This brings us to another fundamental principle,—the higher the rank of the group present in one country and absent in another, the more fundamental is the distinction between them. Thus two adjacent islands might contain not a single Lepidopterous species in common; yet if all the species belonged to genera common to both islands we should rank both in the same region, sub-region, province, and district. But suppose that they had no genera or no families in common, we should consider it necessary to refer them at any rate to distinct sub-regions. If, again, the very orders are distinct, as is the case if we compare the mammals of Australia with those of the rest of the world,* we

* With the exception of the opossums of North and South America.

have before us a distinction of the highest order. But here is a fresh difficulty: it is only Australia which offers us so sharp a demarcation, and even this extends merely to the Mammalia: its birds and insects, though very distinct, not being separated from those of other parts of the world by so broad a boundary line. In separating region from region we cannot always avail ourselves of characteristics absolutely equal in value. This, as we shall afterwards see, has led some systematists to maintain that Australia and South America are marked off from each other and from the rest of the world by features more striking than those presented by any other region. We must therefore call in another principle, already shadowed forth in the admission that mere poverty of species cannot constitute a zoological region. We must take into consideration richness and variety of forms, as well as speciality. Nor must we insist upon being able to prove that all our primary divisions are of precisely equal rank. Nature will not adapt itself to our systematic classifications, whether geographical or morphological. Look, *e.g.*, at our use of the term "order." It is applied equally to two such groups as Carnivora and Marsupialia. It must be admitted that the latter comprises at least four groups which, if more developed, might claim to rank as distinct orders. Or let us look at that vast assemblage of animated beings known as the "order" Coleoptera, but containing carnivorous, omnivorous, frugivorous, and lignivorous groups, differing widely as well in structure as in habits. Were they bulkier creatures, would not the "stirps" Geodephaga be entitled to the position of an order equivalent and parallel to Carnivora? Thus we see that our morphological groups, as well as our geograpical regions, are by no means equal in value.

But to return: the question next arising concerns the foundation of our regional division. Shall it be founded upon the consideration of some one sub-kingdom or class, and, if so, upon which? Mr. Wallace, like some of his predecessors, takes the Mammalia as his standard, and only calls to his aid the distribution of other groups to determine doubtful points, or by way of corroboration. We cannot help thinking that insects have a higher claim to be selected for our guidance. They form, so to speak, the round numbers of the world's animal species, all other tribes and classes being in comparison a mere fractional amount: they are rarely purposely introduced by man into foreign countries, and the few which follow him parasitically, such

as the cockroach and the house-bug, are well known. If imported in articles of commerce they prove, as a rule, incapable of maintaining themselves, and soon disappear. Like the Mammalia, their means of dispersal are mainly dependent upon "the distribution of land and water, on the presence or absence of lofty mountains, desert plains, and great forests." Strange as it may seem, we can also trace their existence and distribution in remote geological epochs, and can identify genera in the tertiary and families even in the palæozoic period. It is true that no part of the world would be so sharply demarcated by its entomological fauna as is Australia by its mammalian forms of life; but this might not be wholly a disadvantage. Yet whilst we wish that an attempt might be made to draw up a system of animal geography based upon the distribution of insects, we are strongly inclined to believe that the main results of such an undertaking would confirm the labours of Mr. Wallace. Even plants will doubtless be found to conform to the same arrangement. "The floræ of tropical America, of Australia, of South Africa, and of Indo-Malaya, stand out with as much individuality as the faunæ, while the plants of the Palæarctic and Nearctic regions exhibit resemblances and diversities of a character not unlike those found among the animals."

Before entering upon an examination of the system of Mr. Wallace we may find it useful to take a brief survey of the divisions proposed by earlier authorities. The first attempts in animal geography are due to Fabricius, the eminent entomologist. He divides the world into eight sections: the Indian, comprising the tropical regions of both hemispheres; the Egyptian, including the northern subtropical lands, apparently in the new as well as in the old continent; the Mediterranean Islands, with southern Europe, and a part of Asia Minor; the North European; the North Asiatic; the North American; China, with Japan; and, finally, all mountains throughout the globe which reach the level of perpetual snow. It does not appear that Fabricius ever made any attempt to demonstrate his theory, which must inevitably break down under even the most superficial analysis, and which merely serves to prove how little attention must have been paid in his day to the localities of animal species. Latreille divides the world into "climates," each extending 12° of latitude by 24° of longitude. He does not, however, express himself satisfied that each of these plots is characterised by a distinct fauna. Kirby maintains that the limits of animal species are fixed

not by isothermal lines,* but by the will of the Creator. He thus withdraws the subject entirely from the domain of Science, forgetting that Absolute Reason will work not arbitrarily, but according to fixed laws, even if the human intellect should not be equal to the task of their discovery.

Dr. Prichard adopts, as his zoological provinces, the Arctic, the Temperate, and Equatorial regions of the old and new continents; the Indian Archipelago; New Guinea, with New Britain, New Ireland, and the island groups of the Pacific; Australia proper; and, lastly, the southern extremities of America and Africa. This classification might be very briefly dismissed if it did not, at first sight, seem to anticipate certain views put forward by Mr. Wallace in his earlier writings, and developed in the present work. He finds that the respective faunæ of the western and eastern portions of the great Malay Archipelago differ essentially: hence he places the former group in his "Oriental" and the latter in his "Australian" region, drawing his line of demarcation between Borneo and Celebes. Whether Dr. Prichard's boundary falls in the same place, or, rather, more to the eastward between the Moluccas and New Guinea, it is evident that he considers the distinction between the Indian islands and New Guinea of no higher rank than that between the former and the south-eastern portion of the Asiatic continent, or than that between the latter and Australia. On the other hand, Mr. Wallace clearly demonstrates that widely as Australia differs from New Guinea in climate, soil, humidity, and state of surface, their respective faunæ show a well-marked affinity. New Guinea and Borneo, almost identical in their meteorological conditions, are decidedly distinct in their forms of animal life. Hence we must decide that Mr. Wallace has not been anticipated by Dr. Prichard, and that the latter was evidently not aware of the importance of the truth which he had approached.

Swainson's arrangement has at least the merit of not requiring any novel terminology. His five grand divisions are simply Europe, Asia, Africa, America, and Australia. How a man of his reading and research could succeed in persuading himself that the zoological distinction between Europe and Asia exceeded, or even equalled, those between Central Asia and India, or North and South America, respectively, might be an interesting puzzle for the laboriously idle. Like Kirby, Swainson supposes that the various

* It will be observed that the boundaries of Latreille's regions are not necessarily isothermals.

groups of organic beings were originally placed by the Creator in certain regions for which they are peculiarly and exceptionally adapted. How completely this hypothesis is at variance with facts requires no further demonstration.

Mr. Wallace, as the basis of his arrangement, adopts the six regions originally proposed in 1857 by Dr. Sclater. This view, at first established merely on a study of the distribution of birds, has since been applied by its author to the mammals, and by Dr. Günther to reptiles. The regions are—the Palæarctic, embracing the eastern continent from the Icy Ocean down to the Sahara, the Indus, and the Himalaya; 2nd, the Ethiopian, including all Africa south of the Great Desert, the tropical portion of Arabia, the islands of Madagascar, Mauritius, Bourbon, the Seychelles, and others in the Indian Ocean, but excluding the Azores, Madeiras, Canaries, and the Cape Verde group; 3rd, the Oriental region, comprising India, both hither and farther, along with the south-eastern portion of China, Ceylon, the Andamans and Nicobars, the Sunda Islands up to the Straits of Macassar, Hainan, and Formosa, and probably the Philippines; 4thly comes Australia, with New Guinea, the Moluccas and Celebes to the westward, and New Zealand and Polynesia to the south and east. Next follows the Neotropical region, or South America, with the West Indies, Central America up to the southern slope of the great Mexican table-land, and the Galapagos. Lastly we have the Nearctic region, including the whole of North America from the Mexican table-land to the furthest limit of animal life in the Polar regions.

Our first thought concerning this arrangement is that its nomenclature is unhappy. Four, if not five, of the names do not at once tell their own tale. Instead of Neotropical and Nearctic, it would surely be simpler to say West-Southern and West-Northern, or even South-American and North-American. The term "Oriental" might be supposed applicable to Persia, Arabia, and Syria, and may therefore be usefully replaced by the name "Indian," as originally proposed by Dr. Sclater. In like manner we would substitute "East-Northern" for "Palæarctic," and "African" for "Ethiopian."

But these six regions, howsoever named, are not universally accepted by naturalists. Prof. Huxley points out that the Australian and Neotropical regions differ more widely from the other four above mentioned than do these latter respectively from each other. Hence if we take Australia, South America, and all the rest of the world, which he calls

"Arctogæa,"—Arctic land, a rather grotesque name for a stretch of country which would have to include the Cape of Good Hope,—we should have only three primary regions, nearly equivalent. The same author also suggests that the peculiarities of New Zealand may perhaps justify its claim to rank as a primary region. Mr. Murray, in his "Geographical Distribution of Mammals," assumes four primary regions,—the Palæarctic of Dr. Sclater, with the addition of the Sahara and Nubia; the Indo-African, embracing Dr. Sclater's Oriental and Ethiopian regions; the Australian; and the American, including South as well as North America.

Mr. W. T. Blanford proposes to call the Oriental region of Mr. Sclater the Malayan, as being most highly developed in the Malay countries. He doubts whether India proper belongs to this region at all, and considers that it has derived a great part of its fauna from Africa.

Mr. E. Blyth, basing his classification upon mammals and birds, seeks to establish seven primary divisions:—the Boreal, comprehending the Palæarctic and Nearctic regions of Dr. Sclater, in addition to the West Indies, Central America, and the Andes down to Chili and Patagonia. Next comes the Columbian region, embracing the residue of South America. His Ethiopian region, in addition to Africa, comprehends Arabia, the south of Syria, the plains and tablelands of India, and even the northern half of Ceylon. Next follows the Lemurian region, comprising Madagascar and the adjacent island groups. Mr. Blyth's "Austral-Asian" region agrees with Dr. Sclater's "Oriental" region if the greater part of India proper is cut off. Finally, Dr. Sclater's "Australian" region is divided into two portions of equal rank—Melanesia, including Australia proper, New Guinea, and Celebes; and Polynesia, comprising the South Sea Islands and New Zealand.

Mr. J. A. Allen, again, assumes a "law of circumpolar distribution of life in zones," and divides the world into eight "realms,"—the Arctic; the North Temperate; the American Tropical; the Indo-African Tropical; the South-American Tropical; the African Temperate; the Antarctic; and the Australian. The North Temperate is again subdivided into the American and the Europæo-Asiatic regions; the Indo-African into the African and Indian regions; and the Australian into the Tropical Australian and the South Australian, with New Zealand. His other realms are not subdivided.

It is evidently disheartening to see such an utter want of

accordance among authors who have certainly given this important subject their careful attention. No one indeed has proposed to unite Australia with any of the other primary zoological regions, or to dissever from it the eastern portion of the Malay Archipelago—the Austro-Malayan region of Mr. Wallace.* But short of this it would almost seem as if zoo-geographers had been "ringing the changes" on the possible number of arrangements. Even with the boldly isolated Australian region strange liberties have been taken: thus while one classifier would confer upon New Zealand the rank of a coequal primary region, another regards it as fit for amalgamation with South Australia, in contradistinction to the northern or tropical half of that continent.

As to most of these modifications of Dr. Sclater's original views, we think that Mr. Wallace is fully justified in their rejection. The fusion of South with North America, or that of the latter with the West Indies, Chili, Patagonia, Central America, Europe, and Asia, seems to us to involve the neglect of important distinctions and of plain affinities, and to offer great practical inconvenience. Mr. Wallace well remarks—"There can be little use in the knowledge that a group of animals is foundin the Boreal region, if their habitat might still be either Patagonia, the West Indies, or Japan." Concerning the proposal of Prof. Huxley—latterly adopted by Dr. Sclater—to consider New Zealand as a primary province, our author reminds us that "it is absolutely without indigenous Mammalia, and very poor in all forms of life, and therefore by no means prominent or important enough to form a primary region of the earth."

"It may be as well here to notice what appears to be a serious objection to making New Zealand, or any similar isolated district, one of the great zoological regions, comparable to South America, Australia, or Ethiopia, which is that its claims to such distinction rest on grounds that are liable to fail. It is because New Zealand, in addition to its negative merits, possesses three families of birds (Apterygidæ living, Dinornithidæ and Palapterygidæ extinct), and a peculiar lizard-like reptile, *Hatteria*, which has to be classed in a distinct order, Rhyncocephalina, that the rank of a region is claimed for it. But supposing, what is not at all improbable, that other Rhyncocephalina should be discovered

* As an additional proof of the close connection between New Guinea and Australia we may mention that M. Bruyn, of Ternate, has obtained a new species of *Echidna* from the mountains of Arfak, in the former island. The only two allied species hitherto known are confined to Australia proper.

in the interior of Australia or in New Guinea, and that Apterygidæ or Palapterygidæ should be found to have inhabited Australia in post-pliocene times (as Dinorthidæ have already been proved to have done), the claims of New Zealand would entirely fail, and it would be universally acknowledged to be a part of the great Australian region. No such reversal can take place in the other regions, because they rest not upon one or two, but upon a large number of peculiarities of such a nature that there is no room upon the globe for discoveries that can seriously modify them. Even if one or two peculiar types like Apterygidæ or *Hatteria* should permanently remain characteristic of New Zealand alone, we can account for these by the extreme isolation of the country and the absence of enemies, which have enabled these defenceless birds and reptiles to continue their existence, just as the isolation and protection of the caverns of Carniola have enabled the *Proteus* to survive in Europe. But supposing that the *Proteus* was the sole representative of an order of Batrachia, and that two or three other equally curious and isolated forms occurred along with it, no one would propose that these caverns or the district containing them should form one of the primary divisions of the earth. Neither can much stress be laid on the negative peculiarities of New Zealand, since they are found to an almost equal extent in every oceanic island."

As regards Prof. Huxley's tripartite arrangement—Australia, South America, and Arctogæa—Mr. Wallace urges that the comparative importance or equivalence of value of two or more zoological provinces is very difficult to determine. "It may be considered from the point of view of speciality or isolation, or from that of richness and variety of animal forms. In isolation and speciality, determined by what they want as well as by what they possess, the Australian and Neotropical regions are undoubtedly each comparable with the rest of the earth. But in richness and variety of forms they are both very much inferior. and are much more nearly comparable with the separate regions which compose it."

It might possibly, however, be contended that Mr. Blyth is right in claiming for Madagascar and its adjacent islands the rank of a primary region, instead of viewing it, with Dr. Sclater and Mr. Wallace, as a sub-region of "Ethiopia." True its extent is very trifling compared with any of the other regions, but there are some grounds for regarding it as the mere fragment of a former continent. It possesses twelve families of terrestrial Mammalia (or only two fewer

than the Australian region), three of which are peculiar, whilst the Lemuridæ—although extending to continental Africa, the Malay Islands, India, and China—have evidently their metropolis in Madagascar, where they take the place of the monkeys. Of its twenty-seven genera and sixty-five species, no fewer than twenty genera and all the species are peculiar. Out of its one hundred and eleven species of land birds only twelve are identical with species inhabiting the adjacent continents, and not one of the exclusively African families is represented in Madagascar. The gigantic extinct bird Æpyornis, three species of which have been discovered, forming the family Æpyornithidæ, as far as is known was peculiar to Madagascar and the adjoining islands. Among reptiles it contains none of the African *Colubers*, but it has three genera—*Herpetodryas*, *Philodryas*, and *Heterodon*—which are only found elsewhere in North and South America, whilst the Lycodontidæ and Viperidæ, both well developed in Africa, are here absent. Its insect affinities are largely Oriental, Australian, and South-American, the African element being represented rather by special forms belonging to West Africa or South Africa than by such as are common to the whole Ethiopian region. Amongst the Lepidoptera the beautiful diurnal moth *Urania* occurs here, the remaining species being found only in the Neotropical region. Of the twenty-three Cetonian genera known in Madagascar, two alone—according to the "British Museum Catalogue"—are represented elsewhere. None of the characteristic *Cicindelas* of Africa are here met with, and with the *Carabs* the case is almost similar. Having regard to these facts, most if not all of which are duly recorded by Mr. Wallace, it certainly seems that "Lemuria," if not a primary region, differs more widely from the other Ethiopian sub-regions than they do from each other, or perhaps even than do the respective sub-regions of any other region.

The subdivision of the six primary regions is the next question. Mr. Wallace proposes in every case four sub-regions, a numerical agreement which, if fully borne out by facts, is exceedingly curious. Doubts, however, may be entertained both as regards the number and the boundaries of these subdivisions. In the Nearctic or North-American region the four districts are—the Californian, consisting of Upper California and the narrow strip of country to the northwards between the Rocky Mountains and the Pacific; the Rocky Mountains, embracing Lower California, the table-lands of Mexico, and the mountainous territories extending northwards towards the British frontier; the Appa-

lachian, including the valley of the Mississipi and all the eastern and southern States of the American Union; and, lastly, the Canadian, comprehending British North America (except perhaps Columbia), the former Russian territory, Greenland and the Polar lands, so far as they have any animal life at all.* These sub-regions, as Mr. Wallace admits, are not so well characterised as might be desired, and their boundaries, and even their number, are therefore open to doubt.

In the Neotropical region the four divisions are—the Andean, comprising Patagonia, the chain of the Andes, and the Pacific Coast up to the Equator; the Central American, with the hot low-lying coasts of Mexico; the Antillean, including all the West Indies except Tobago and Trinidad. The remaining sub-region comprises these two islands, Guayana, Venezuela, and the greater part of New Grenada and Ecuador, those portions of Peru and Bolivia which lie on the eastern slope of the Andes, the vast empire of Brazil, and a part of Paraguay and the States of La Plata. This arrangement has not escaped criticism. It has been pointed out that the last sub-region, superficially very large in proportion to the three others, and greater still in the amount of fruitful land which it contains and in its multitude of animal species, may probably be found less homogeneous than Mr. Wallace supposes. We do not think that the attention of naturalists has been sufficiently directed to a passage in which Sir R. Schomburgk, speaking of the brilliant flora of the mountains of British Guayana, notices the almost total absence of insects. The remark has been made that this statement, if confirmed, agrees ill with certain modern views on the part played by insects in the fructification of plants and on the use of brilliant colouration in flowers. Waterton, whose testimony would have been invaluable, unfortunately paid little attention to insects.†

It is instructive to compare the West Indian islands with the Malay Archipelago. Both these groups are closely

* We have always been of opinion that the extremely high latitudes would be found completely devoid of a fauna, and we observe with much interest that this view is decidedly confirmed by the results of the late Polar Expedition. The explorers appear to have reached, and even passed, the boundaries of animal life. The bearing of this fact upon the notion of a circumpolar zoological region is obvious.

† Mr. Norman Moore, in his edition of Waterton's "Essays," declares that Schomburgk "has copied whole pages from the 'Wanderings' with no other change than the transformation of an interesting into a heavy style, and, notwithstanding all his obligations to Waterton, he has never once mentioned him in his books with respect."

similar in soil, climate, and possible productions, but the poverty of the Antilles, whether in the higher or lower forms of animal life, is as remarkable as the wonderful riches of the Eastern islands.

Turning to the Australian region we find again four sub-regions :—Austro-Malaya, or New Guinea ;* Celebes and the Moluccas ; Australia proper, with Tasmania, New Zealand ; and, lastly, the South Sea Islands. Mr. Wallace doubts, however, whether the Sandwich Islands are not entitled to rank as a fifth sub-region, distinct from the rest of Polynesia, and having affinities which apparently point to a former connection with the western coast of North America. Concerning the Philippines, he is also uncertain whether they should be ranked with the Austro-Malayan sub-region or assigned to the Malayan section of the Oriental region. This being the case it is the more to be regretted that the author omits to furnish an analysis of the insect forms of this interesting group.

The distribution of animals, recent and fossil, in the Australian region is the more important since from its study we may hope to obtain light on such questions as the former existence of a vast equatorial continent of which the Polynesian islands are the mountains, or of an antarctic continent of which New Zealand may have formed an outlying portion, or of a former eastward extension of Australia and its connection with or approximation to New Guinea.

The divisions of the Oriental or Indian region are—the Malayan, comprehending the Sunda Islands, with the peninsula of Malacca, and possibly the Philippines ; the remainder of further India, with the southern coast of China, and probably the islands of Hai-nan and Formosa; southern Hindustan, with Ceylon ; and northern Hindustan. The position of Hai-nan, and especially of Formosa, is somewhat doubtful. Its fauna has, on the one hand, affinities with that of China (Palæarctic region), and, on the other, with those of the Indo-Chinese, and even the Malayan sections of the Oriental region. Here, again, it is to be regretted

* Reports have been spread which if verified would have necessitated the total exclusion of New Guinea from the Australian region. Thus one traveller declared that he had met with the recent dung of a rhinoceros, and had seen the track of buffaloes in the mire. A more competent observer, however, Signor D'Alberti, has shown that the dung was merely that of the *Casuarinus*, and that the tracks had been made by wild swine. New Guinea is remarkable for the beauty of its insects and of its birds. According to Mr. Wallace fully 50 per cent of the latter are brilliantly coloured, whilst in such districts as Malacca and the Valley of the Amazon the proportion is not above 33 per cent.

that no information has been given concerning the insects of the two islands.*

The Andaman and Nicobar groups have been assigned, the former to the Indo-Chinese and the latter to the Malayan region, an arrangement which we think no judicious zoo-geographer will call in question.

But a much more serious question remains. Certain naturalists, who have made the fauna of India their especial study, hold that its affinities with Africa overbalance those with Malaya. Mr. Crawford maintains that this Ethiopian affinity was more pronounced in the tertiary period than it is at present. It is further urged that as there is easy communication for birds, and even for mammals, between India and Indo-China, or even Malaya, but long tracts of sea and desert between the former and the Ethiopian region, the affinities of India and Africa ought to be estimated at a higher value than if the means of access were equal in each case. Still, as far as the Mammalia are concerned, we question if the affinities of the Indian sub-region for Africa are more pronounced than those for Malaya, or even for the Palæarctic territories. Bears and deer occur in India, but have not been met with in Africa, either living or fossilised. Of the thirty-eight mammalian genera inhabiting India proper, eight are so widely distributed as to give no special clue to the question; "fourteen are exclusively Oriental; five have as much right to be considered Oriental or Ethiopian, extending as they do over the greater part of the Oriental region; two (the hyæna and gazelle) show Palæarctic rather than Ethiopian affinity; seven are Palæarctic and Oriental, but not Ethiopian; and only two (the hunting leopard and the *Mellivora*) are distinctly Ethiopian."

The Ethiopian region, if we include Madagascar, is also divided into four sub-regions, which, however, Mr. Wallace regards as "in some extent provisional." The East-African sub-region includes all the open country south of the Great Desert, and extending eastwards to the Indian Ocean, and southwards to about 20° S. latitude. It has few peculiar forms, and its north-eastern regions are almost as much Palæarctic as Ethiopian; while the fauna of the forests of Mozambique, though on the eastern coast, approaches in character to the western or southern sub-region. The western district includes the mass of forest which lies in the

* A large amount of our knowledge on the distribution of animals is due to sportsmen, who often do excellent service as regards mammals and birds, but who generally overlook the reptiles, and almost invariably ignore the insects.

form of a crescent along the Gulf of Guinea, having its northern limits at the River Gambia; extending eastwards to the head-waters of the Nile and the mountains forming the western boundary of the great lakes of Central Africa, and reaching southwards "to that high but marshy forest country in which Livingstone was travelling at the time of his death." Its extreme southern limit may be about 11° S. latitude. The author tells us in a footnote that Dr. Schweinfurth found this region very sharply defined in 4½° N. lat. and 28½° E. long. A sudden change occurs here in the character of the vegetation, and the chimpanzee and the West-African grey parrot first make their appearance. The South-African sub-region, which Mr. Wallace pronounces "the most peculiar and interesting part of Africa," occupies the extreme south of the continent, as far as the Kalahari Desert and the Limpopo River. The more typical portion of the region scarcely extends beyond the boundaries of the Cape Colony and Port Natal.

There is evidently much to be done before the fauna of Africa can be mapped out with even approximate exactness. Possibly some of its variations may be considered cases of "station" rather than of "habitat." The western district is a luxuriant forest, whilst the eastern is to a great extent elevated table-land, with a vegetation of high grasses, thorny scrub, and here and there patches of forest. It is very intelligible that the animal population of two such districts should show a well-marked difference. We need not wonder, therefore, that the forests of Mozambique should possess a fauna more western in its character, and that the Ethiopian affinities of the Indo-Malayan sub-region should point more to Western than to Eastern Africa.

The Palæarctic region, lastly, is divided into the following four sub-regions:—the European, comprising all the land north of the Pyrenees, the Alps, the Balkan, the Black Sea, and the Caucasus, and west of the valley of the Irish and the Caspian, and including the British Islands, "whose animal productions are so uniformly identical with continental species as to require no special notice." The Mediterranean sub-region includes Spain, Italy, Turkey (European and Asiatic), Persia and Affghanistan up to the banks of the Indus, Northern Arabia, Egypt to the second cataract of the Nile, Northern Africa so as to include the extra-tropical portion of the Sahara and the Azores, Madeiras, Canaries, and even the Cape Verde Islands. It may at first sight seem strange that the northern and southern shores of a deep sea like the Mediterranean should belong to the same

sub-region, since deep seas generally mark out a primary division in the faunæ of the lands they separate—a truth which Mr. Wallace has enforced and utilised in his work on the Malay Archipelago. But the Mediterranean, though a very deep sea, did not always form a continuous barrier between Europe and Africa. It was bridged over at Gibraltar, and again at the part between Sicily, Malta, and the African coast, and thus an easy communication between its northern and southern shores was possible. The traveller who proceeds from Spain or Italy, either southwards or eastwards, finds no very marked transition until he has reached the Niger in the one direction or the Indus in the other. The faunæ of the Azores, Madeiras, and Canaries have been carefully examined, and show unmistakable Palæarctic affinities, having been derived at an early period either from South-Western Europe or North-Western Africa. This circumstance speaks against the supposition that these island-groups are the last remains of a former continent (Atlantis), either independent or connected with tropical America. In either of these cases their faunæ would have exhibited a marked distinction from that of Mediterranean Europe.

The third Palæarctic sub-region, the Siberian, occupies the whole of Northern, North-Eastern, and Central Asia as far as the frontiers of the Oriental region. Middle and Northern China and the islands of Japan form the fourth and last sub-region.

Each of the six great regions "has had a history of its own, the main outlines of which we have been able to trace with tolerable certainty."

The question now arises, what are the causes of the distribution of animals as at present existing—a distribution which mere differences of temperature, of moisture, and of the supply of food are far from fully explaining?

Among the agencies which have influenced the migrations and re-migrations of species, a prominent rank belongs to the Glacial epoch—perhaps we might rather say the Glacial epochs. It is important to note that on this subject Mr. Wallace accepts the views of Mr. Belt. This acute geologist holds that glaciation was simultaneous over both hemispheres, and that the amount of water piled up in the form of ice upon the continents so far lowered the depth of the ocean as to lay dry extensive tracts of land, now submerged to the depth of 2000 feet, these low-lying regions becoming the refuge for tropical forms of animal and vegetable life. It is scarcely necessary to add that this hypothesis would never have secured suf-

frages so exalted if it did not harmonise well with the phenomena of the variation and distribution of species. But there are undoubtedly difficulties in the way. A simultaneous glaciation of the whole earth, preceded or followed by a period like the Miocene, with a luxuriant vegetation penetrating at once almost to either Pole, implies a wonderful oscillation in the total available amount of heat at the earth's surface.

Another important cause has been the fluctuation in the distribution of land and water on the earth's surface. It is true that on this subject exaggerated views have been entertained. Mr. Wallace does not consider it either justifiable or necessary to assume that—at any rate in recent geological periods—the great masses of land and of water have changed their respective positions; but he holds that along the margins of the continents great mutations may have taken place. Northern Asia and the Sahara are probably recent upheavals of shallow sea-bottoms. Borneo, Java, and Sumatra may probably have been united with further India, and again separated; Australia may have very probably extended to the great barrier reef, and the Antilles may have been part and parcel of the American continent. Nay, at a very early period Madagascar may have been connected with Africa, and subsequently with Ceylon and Southern India.

Such changes would follow naturally from the conditions assumed in Mr. Belt's hypothesis of the Glacial period; but Mr. Wallace decidedly opposes the theory of a former direct communication either between Africa and South America or the latter and Australia. The occurrence of certain peculiar forms of life in countries mutually very remote, and their absence in intermediate regions, may, he shows, be explained much more naturally than by the assumption of direct and special connection. Such species may have been once very widely diffused, but being worsted in the great struggle for existence they have been exterminated, save in remote islands and inaccessible districts. Such species it will be found are rarely powerfully armed. As a confirmation of this view, it may be noticed that the strongest and most formidable animals are found either in the large continents or in the islands recently dissevered from them, such as Sumatra or Java. The faunæ of true oceanic islands, on the contrary, are characterised not merely by scantiness, but by the general feebleness and inoffensive character of the species. A very curious fact, to which Mr. Wallace more than once refers, is the evident extinction of many large species

of animals since the post-pliocene times, and either just before or shortly after the appearance of man. This phenomenon was apparently simultaneous in Europe and America, and will doubtless be traced in other quarters of the globe when their palæontology has been more fully studied. Among the animals which have thus disappeared are some which seem to have been eminently qualified to "hold their own." Such is the extinct genus *Machairodon*, or sabre-toothed tiger, several fossil species of which, apparently more formidable than any existing cat, have been found in both continents.

Another very important conclusion is, that the theory of the independent origin and development of animal life in a number of distinct points can no longer be upheld. The great northern continents appear to have been the seat and birthplace of all the higher forms of life, whilst certain strange creatures—such as the gigantic fossil Edentata—seem to have originated in the south and to have gradually spread northwards.

In one respect we must own ourselves disappointed with Mr. Wallace's book, although the author, in his Address delivered before the Biological Section of the British Association at Glasgow, has done very much to supply the deficiency. We had expected that the work would have contained a summary of facts, and possibly some interesting generalisations on the influence of locality on the colour, the size, and the form of animal species. Every naturalist —save such, if they deserve the name, who confine their researches to books—knows that the fauna of each country has a peculiar general physiognomy, more or less pronounced. These peculiarities are sometimes difficult to express in words, and may escape any but the most patient observer, but in other instances they are open and palpable. Thus, according to Mr. Goodman, there appear in the birds of the Azores modifications all tending towards a more sombre plumage, and a greater strength of feet, legs, and bill. Mr. Blanford finds that Persian specimens are, on the average, paler in colour than their nearest allies in Europe. Mr. Wallace, in his Glasgow Address, remarks that it is "in islands we find some of the most striking examples of the influence of locality on colour, generally in the direction of paler, but sometimes of darker and more brilliant hues, and often accompanied by an unusual increase of size." He then shows how the butterflies of certain genera, such as the *Euplæas*, are in the larger islands dark-coloured, whilst in Banda, Ké, and Matabello there are three

species, not nearly related to each other, "all broadly banded or suffused with white." "The small island of Amboyna produces larger-sized butterflies than any of the larger islands which surround it. This is the case with at least a dozen butterflies belonging to many distinct genera."

Corresponding cases of paleness are found in Fiji, in the Andamans, and in Jamaica.

In Celebes, as Mr. Wallace has shown in an earlier work,* instead of any modification of colour, there is "a peculiar form of wing and a much larger size running through a whole series of distinct butterflies."

The Philippine Islands seem to have the property of developing intense metallic lustre, both in butterflies and beetles. Thus the hind wings of *Ornithoptera Magellanus* "glow with an intense opaline lustre not found in any other species of the entire group, and *Adolias calliphorus* is larger and of more brilliant metallic colouring than any other species in the Archipelago. In these islands, also, we find the extensive and wonderful genus of weevils, *Pachyrhynchus*, which in their brilliant metallic colouring surpass anything found in the whole eastern hemisphere, if not in the whole world."

Continental districts likewise have their peculiarities of colouration. Thus two unrelated groups of butterflies from tropical Africa "are characterised by a prevailing blue-green colour not found in any other continent." Similarly, in South America, "nine very distinct genera are implicated in parallel changes, groups of three or four of them appearing in the same livery in one district, while in an adjoining district most or all of them undergo a simultaneous change of colouration or marking."

These local peculiarities may perhaps be overlooked because they occur in insects, but Mr. Wallace very aptly asks—What should we think if similar phenomena were to be traced amongst large mammals?

In birds, however, local characteristics of a corresponding nature occur, as the author proves by reference to the avifaunæ of the West Indies, the Andamans, the Philippines, Celebes, Timor, Flores, and Lord Howe's Island. In New Guinea, Australia, Madagascar, and the Mascarenes we have black parrots and pigeons—a curious instance of the phenomenon known as melanism.

* Contributions to the Theory of Natural Selection, pp. 168—173.

It has been maintained that, in the Nearctic region at least, the Mammalia increase in size with the latitude and altitude of their birthplace. This view is disputed by Mr. Allen, who declares that it does not agree with the development of the American Carnivora. Indeed as regards altitude, and consequently rarefaction of the ambient medium, the very opposite law has been proposed. The largest animals are now found in the denser medium, water; many of the next largest species—such as the elephants, hippopotamus, &c.—inhabit river-marshes and deltas almost on a level with the sea. There is also good reason for supposing that the atmosphere in the epochs which produced the gigantic extinct animals must have been denser than it is at present. Mr. Allen considers that the largest individuals of every species, and the largest, best-developed, and most typical species of every group, will be found near its centre of distribution.

It has also been suggested, at least as regards insects, that in all regions dark-coloured species are characteristic of woods, whilst white or light-coloured forms occur in the open plains. This law holds good in some well-known genera of butterflies, such as the "whites" as compared with the forest-loving *Hipparchias*, *Erebias*, &c.; but the deeply-coloured *Vanessas* inhabit the open country. Among Coleoptera we find the dung-beetles, mainly black in colour, abundant in the open country, like the ruminants on whose excrement they prey. The common ground-beetles, also (Harpalidæ, &c.), are chiefly sombre in colour, and certainly show no exclusive preference for woods. On the other hand, the chafers (Melolonthidæ) and the Buprestids frequent the woods, and yet display some of the most striking instances of pale colouration to be found among the entire tribe of Coleoptera.

For further details on the influence of locality we must refer the reader to Mr. Wallace's most instructive Address, but we cannot help expressing our regret that he has not introduced the consideration of this subject into his *magnum opus*.

We may rest assured that the peculiarities to which we have so briefly referred point to causes of variation at work other than protective mimicry or than sexual selection. This is evidently the opinion of Mr. Wallace, who insists that "one of these causes is an influence depending strictly on locality, whose nature we cannot yet understand, but whose effects are everywhere to be seen when carefully

searched for. If it be asked why so little attention is given to this and to other interesting problems connected with the distribution of organic life, one important cause has been pointed out by Mr. Wallace—the total want of a museum geographically arranged. Our museums, more or less complete, are arranged morphologically. Birds, reptiles, insects, &c., which approximate in their structure are placed together, quite irrespective of the locality from which they have been obtained. Such collections are obviously indispensable, and all we recommend concerning them is that they should be made much more complete and more accessible than is now the case, and should invariably be placed in some central position, and not in a remote, even though fashionable, suburb. But along with such we want also a museum geographically arranged, where we may see, *e.g.*, in one hall the fauna of India, in another that of New Guinea, of Australia, of Madagascar, or of the Cape. Who can doubt that if such collections were accessible, relations, similarities, contrasts would strike us which escape unnoticed when the species of the whole world are placed side by side. It would likewise be instructive to exhibit the species of every country in juxtaposition with their nearest allies or representatives in other countries, and to show specimens of widely-distributed species from the centre and the extremes of its range. Surely if any nation can produce such an institution it ought to be England; yet hitherto we have little even pointing in this direction save the collections of "British" birds and insects, which have been multiplied both by public institutions and by private collectors, and which are the less instructive because Britain is not a definite zoological district, but merely an impoverished portion of the north-western Palæarctic region. Is it too much to hope that the great Colonial Museum which looms in the future, and which by the special favour of all good powers is to be placed on the Thames Embankment, may include a department of the kind desired?

We must now, however, take our leave of Mr. Wallace and of his truly magnificent contribution to Natural History. If we cannot pronounce the work as in all respects perfect—if we here and there entertain a doubt or desire fuller information—the cause lies not in any shortcoming on the part of the author, but in the extent and the complexity of the subject and in the limited state of our present knowledge. The plan which he has traced with so masterly a

hand will doubtless be elaborated in its details, and may perhaps here and there be somewhat modified. But we shall all feel that Alfred Russel Wallace is the architect whose designs we are carrying out.

V. ON THE LOESS OF THE RHINE AND THE DANUBE.

By Thomas Belt, F.G.S.

THE sides of the valleys of the Rhine and the Danube, as well as those of their tributaries, are covered with a yellowish grey calcareous clay up to a height of several hundred feet above the rivers. This clay is often of great depth, is unstratified, and contains land and fresh-water shells scattered throughout it. It also contains the bones and the stone implements of palæolithic man, and the remains of the mammoth, the woolly rhinoceros, and other Mammalia. This clay has been named the Loess, and respecting its origin and its relation to the excavation of the great valleys there is much difference of opinion.

Sir Charles Lyell, in commencing an able summary of the facts respecting the distribution of this deposit, states that skilful geologists peculiarly well acquainted with the physical geography of Europe have styled the loess the most difficult geological problem.* It is certainly curious to contrast the ease with which some philosophers map out the world in former geological periods, showing with confidence which areas have been beneath the sea and which remained dry land, with the difficulty that is experienced in getting any idea of what was the condition of the continents during the formation of the latest deposits, after man had penetrated into Europe, and all the species of animals and plants now living had come into existence. And yet, until we understand the youngest of the geological formations, it is almost idle to speculate on the more ancient conditions of the earth's surface. Whilst, for instance, it is held by many that much of northern Europe and America was depressed below the level of the sea in post-tertiary times, and rose

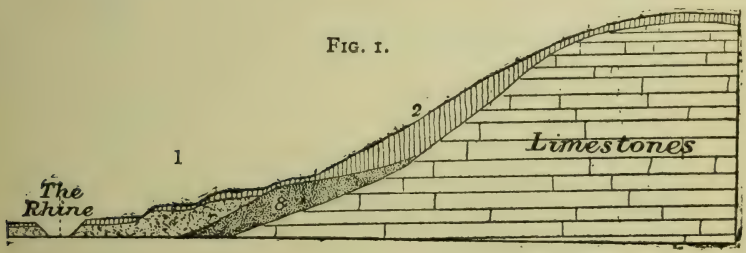
* Antiquity of Man, 4th edition, p. 372.

again to its former and present level, yet that the deposits formed beneath the waters of that ocean contain no marine organisms over thousands of square miles, what confidence can we feel in those theories that map out the continents in older geological periods according to the presence or absence of the remains of the inhabitants of the sea. The uncertainty felt respecting the latest formations fully justifies the large share of attention now paid to them and the efforts that are being made by many minds to bring the facts within the range of some consistent theory. This is not a case where the facts are unknown. No other formation has been so much studied as the quaternary. What is needed is a consistent theory of origin which shall violate no natural laws, be in harmony with the older geological record, ask for the aid of no unknown force, and in which every fact shall fall into its natural place and find its full explanation.

Impressed with the importance of the question, I have during the last twelve years endeavoured to make myself thoroughly acquainted with the facts, and have had unusual facilities for studying them in both hemispheres. When known, they appear to marshal themselves into a certain order, and to suggest a theory of origin that I have supported in the pages of this journal and elsewhere, but for which I have not yet obtained much consideration from geologists. My business occupations not leaving me sufficient time to prepare a general treatise, in which the various questions might be fully worked out and their relations to each other shown, I have been obliged to present my theory in short essays on distinct groups of facts, and in the present paper I propose to take into consideration the difficult question of the distribution of the loess, and to show that it is explained by the theory that at the culmination of the Glacial period the ice principally occupied the ocean depressions, and blocked up the drainage of the continents as far as it extended.

The loess, in the valleys of the Rhine, the Danube, and their tributaries, is found up to a great height above the present rivers. That of the Rhine is well illustrated around Basel, and the following section (Fig. 1) is one of many that may be seen in that neighbourhood. It is taken on the right bank, where a low range of vine-clad hills comes down nearly to the river, about 2 miles east of the city. Quarries of limestone are worked in the hill-side, and fine sections of the loess exposed. It is here a grey calcareous clay, of such a firm consistency that the workmen have excavated a chamber in it to keep their tools in ; and this is its general

character wherever it is seen. Land shells are scattered throughout it, and I found them up to a height of about 1110 feet above the sea, which was as high as there were good sections of it exposed; but above this I traced it to the top of the hill, or to a height of about 1470 feet above the sea and 660 feet above the river. I took my altitudes by means of an aneroid barometer, calculating from the known height of the river at the bridge at Basel (803 feet), and though not absolutely correct they may be relied on to within a few feet, as I returned to my starting-point and found that only a small change had occurred in the barometric pressure in the meantime. Besides the land shells, small angular pieces of the local limestones and calcareous concretions are distributed throughout the loess. At the base of the hill it rests on a bed of gravel that has been



Section near Basel.

1 Terraced gravels. 2. Loess. 3. Old gravels.

cemented into a conglomerate by calcareous infiltrations. This gravel is composed of pebbles of crystalline rocks, rounded and subangular, and ranging in size from 1 inch up to 4 inches in diameter. With these are mixed angular and subangular fragments of the local limestones, of greater size. The highest point at which I noticed this old gravel-bed was 960 feet above the sea. It rests against the old valley bank, and the loess is distinctly superimposed upon it, and contains in its lowest part many pebbles derived from it. The importance of this fact will be recognised when I come to the discussion of the question of the origin of the loess.

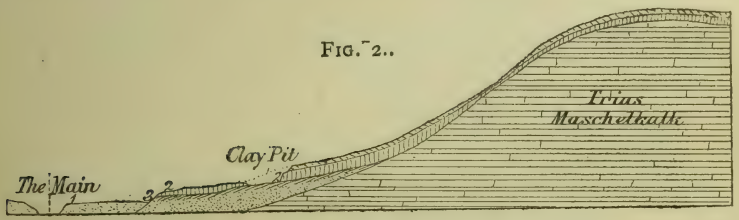
Between the base of the hill and the river there are three terraces of gravel capped with clay, at the heights respectively of 923, 860, and 830 feet above the sea, the lowest being about 25 feet above the river. I think that these may have been of more recent origin than the loess, and that the

clay with which they are covered may have been washed down from the hills above them.

The loess may be traced all round the flanks of the hills that surround the great plain between Basel and Bingen, and also on the hills that in some places rise up like islands in its midst. Thus Sir Charles Lyell notes that it covers, up to a height of 1600 feet above the sea, the Kaiserstuhl, —a volcanic hill which stands in the middle of the great valley of the Rhine, near Freiberg, in the Brisgau. It is also spread over the volcanic hills of the Lower Eifel. Dr. Samuel Hibbert, in his "History of the Extinct Volcanoes of the Basin of Neuwied on the Lower Rhine," published in 1832, gave many instances of the intercalation of the loess with beds of volcanic ashes near Andernach. Much white pumice is spread over the loess, and it seems to be well ascertained that both during its deposit and afterwards the volcanoes were active. The veteran geologist, Herr Henry von Dechen, has mapped out these deposits, and described them in his "Geognostischer Fichrer zu dem Laacher." He has informed me that one of the highest patches of the loess on the volcanic hills lies south of Andernach, near the hill of Korrets, at an altitude of about 620 feet. Dr. Hibbert appears to have found it higher, for he states that on the Mahlsberg the loess attains an elevation of 800 feet above the sea, or 600 feet above the river, and is sometimes 60 feet thick. With regard to the pumice that is abundantly spread out on the top of the loess between Andernach and Coblentz, and which Herr von Dechen informed me indicated that the volcanoes were active at the time of the close of the deposition of the loess, it may be worth remarking that the *Challenger* explorations have shown that great quantities of pumice are spread over the bed of the ocean, proving it to be a common product of submarine volcanoes; so that it is quite in accordance with our knowledge of its production to suppose that the volcanoes of the Eifel at the time of its eruption may have been submerged beneath the waters from which the loess was deposited.

The loess extends up the valleys of the tributaries of the Rhine. The basin of the Neckar is, according to Sir Charles Lyell, filled with it. It is of great thickness, and at Canstadt, near Stuttgart, overlies a bed of gravel, and contains bones of the mammoth and the woolly rhinoceros. There are thick deposits of loess in the valley of the Main. I had the great advantage of examining it near Wurzberg, in the company of the celebrated geologist Dr. Sandberger. It has been principally preserved, from the effects of great

floods that appear to have swept down the valley, in bays and recesses, and at these points bones of the mammoth and its associates have been found. I took the following section (Fig. 2) at Blosenberg, near Heidingsfeld, where the loess is extensively dug for brick-making. At the clay-pit the lowest bed seen is one of subangular quartzose gravel, with pebbles of crystalline rocks from the mountains at the sources of the Main. This is covered with clean false-bedded sands, containing lines of small angular and subangular pebbles, mostly of quartz. The sands are covered by loess, which at the base is sandy and a little stratified, but soon graduates upwards into unstratified calcareous loess with vertical joints. It is divided into two beds by a clear line of division, the upper one of which is of a lighter colour than the lower. In another section, in a small valley to the



SECTION NEAR WURZBURG.

1. Alluvial plain. 2. Loess, with rubble and reconstructed loess on top.
3. Gravels and false-bedded sands underlying loess.

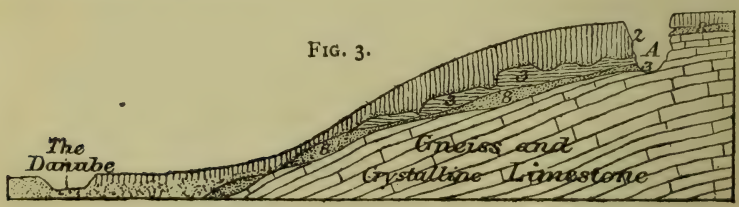
south of Marianberg, the division line is irregular, and is strongly marked by the occurrence along it of angular fragments of limestone.

At Blosenberg the loess conforms to the configuration of the ground, and in all the sections I saw, the different divisions were inclined with the slope of the hill, and had not been deposited in horizontal strata that had been afterwards denuded, but rather as a mantle on the slopes of a pre-existing valley. Shells of land mollusks are very abundant at Blosenberg, particularly on the slopes of the hill, where there are continuous sections by the sides of the deeply-cut paths that lead up through the vineyards. They occur here mostly in lines that give a sort of stratification to the loess. These lines of shells rise with the slope of the hill. I traced them up to a height of about 670 feet above the sea, and the loess up to 714 feet. From thence upwards the steep slope is mostly covered with limestone *débris* until we get to the summit, which is again covered with

loess. On the northern slope of the hill I found a thin layer of characteristic loess, beneath local *débris*, up to a height of 880 feet above the sea. On the hill-tops it occurs to 1100 feet or more, and Dr. Sandberger informed me that it is sometimes thick on the hill-tops, and contains the characteristic loess shells, though not so abundantly as lower down in the valley. I am inclined to think that it was originally deposited continuously over the slope up to the tops of the hills, but has since been removed by denudation, as the places where it is absent are just those where it would be most likely to be washed off, and it appears to be present wherever the ground becomes flatter.

In the basin of the Danube the loess is more generally and thickly spread out than even in the watershed of the Rhine. Prof. Edward Suess has kindly furnished me with much information respecting its distribution. It is found up to a height of at least 1300 feet above the sea in the upper part of the valley of the Danube. Where the river issues from the narrow gorges that it has cut through the crystalline rocks that range southwards from Bohemia, and again through the northern prolongation of the Alps above Vienna, the loess is heaped up as if deposited at the head of a lake, and just as the mud of the Rhone is now being deposited at the upper end of the Lake of Geneva. I visited Krems, where the valley—greatly contracted above where the river passes through gneiss and other crystalline rocks—widens out into a great plain, and there I found, as Prof. Suess had before informed me, the loess heaped up in a cone around the expanding mouth of the gorge. A little below the town of Krems, on the left bank of the Danube, I got the following section :—

FIG. 3.



SECTION NEAR KREMS.

2. Loess, with gravel and boulders at base and reconstructed loess on top. 3. Stratified sands and clays. 8. Old gravels, Miocene. A. Ravine showing pre-diluvial cliff.

The low hills bounding the valley are all covered with loess, and sections are exposed showing a thickness of over

60 feet without its base being seen. Many bones of the mammoth have been found in it, and it is related that when in the thirty years' war the Swedes were besieging Krems they found in one of their trenches the skeleton of a monstrous animal, and that besiegers and besieged ceased from their warfare for a time to gaze on the huge teeth of the giant that had been dug up.* Some bones were shown to me at the Imperial Museum, by Prof. Fuchs, which bore cuts and indentations apparently produced by a cutting instrument, and which are supposed to have been made by man. There is no reason to question this inference, as a few palæolithic implements have also been found; and that man was a contemporary of the mammoth is now fully established.

In ascending one of the many roads leading up the vine-covered slopes I found deep sections of the loess exposed, and it often rises like a wall on each side to a height of from 20 to 40 feet. The roads appear to have been purposely cut down deep, to allow not only wine-cellars but dwelling-houses to be excavated in them. The loess at Krems is compact, and almost like chalk in its homogeneous consistency, without the joints and fissures of the latter. It contains patches and seams of gravel, and pebbles of quartz are irregularly scattered throughout it. It is unstratified, but occasionally lines of division are seen separating portions of slightly different colour and composition. At its base I saw much gravel, and some boulders resting on the denuded top of stratified sands and clays. At one point I found that it had been banked up against a pre-diluvial precipice, as shown at A in Fig. 3.

In some parts patches of miocene gravels lie below the stratified sands or clays, and in these the remains of *Mastodon longirostris* have been found. Prof. Suess informed me that he had determined that these gravels were heaped up in Miocene times in the same parts of the valley where in the Quaternary times the loess was most thickly deposited, and he thinks that at that early period the valley had assumed in a great measure its present configuration.

The loess is by no means confined to the valleys of the great rivers and their tributaries. Sir Charles Lyell† and Mr. Godwin-Austen‡ both follow the Belgian geologists in identifying it with the "Limon de Hisbaya," which covers much

* I am indebted to Prof. Edward Suess for this anecdote.
† Antiquity of Man, p. 329.
‡ Quarterly Journ. Geol. Soc., vol. xxii., p, 251.

of Belgium, enveloping Hainault, Brabant, and Limburg like a mantle. It extends into France, and covers the high plains between the rivers, and is most likely the same as the upland loams of the valleys of the Seine and the Somme, which occupy positions often independent of the present lines of drainage. It is even probable that it once covered the south-east of England, but has since been denuded, as Prof. Morris has shown to me some of its characteristic shells which he had gathered in patches of loess, preserved in fissures of the chalk, near Maidstone.

There is also much evidence tending to prove that the loess is the equivalent in the river valleys of the northern drift that covers so much of Northern and Central Europe. Thus in ascending, from Vienna, the valley of the March, I found all the flanks of the hills covered with a loess like clay, which as I travelled northward changed to a redder colour. This clay covered the low watershed between the Danube and the Oder, to a depth of 20 feet in some places. The watershed is only about 1000 feet above the sea, and the clay covers it and follows along the flanks of the hills on either side. On the northern flanks of the range, Scandinavian blocks, that must have been carried from the far north, are found up to heights of over 1200 feet. These occur all along the northern side of the Carpathians. The diluvial clay extends much higher. According to Prof. Stur it fills the valleys on the north slope, and I have always found it extending to much higher levels than the northern blocks. This is what we might expect, for the icebergs that brought the blocks from the far north must have required a considerable depth of water to float them.

In 1875, at Wolochisk, in the province of Volhynia, near the Russian frontier, whilst detained for the examination of luggage and passports, I found, in the clay that had been thrown out of a well, shells of *Succinea oblonga* and *Pupa muscorum*, two mollusks that have left their shells in the loess almost everywhere where it occurs. This was not in a valley, but on the top of the flat plateau that forms the steppes of Southern Russia. I was much impressed with the occurrence of these characteristic loess shells at this place, as no higher ground overlooks the plain, and the clay may be followed continuously for hundreds of miles, and to the north contains transported Scandinavian rocks. On my return from a visit to Southern Russia, during the present year, I determined to examine this clay, which is "the diluvium" of the Russian geologists, nearer to the mountains, and for that purpose stayed twelve hours at Podwolochisk,

in Galicia. I was greatly gratified by finding loess shells in abundance, and thus adding another link to the chain of evidence that connects the loess with the northern drift. This northern drift and the clay that accompanies it is the Upper Boulder clay of the east of England; and in 1864 Mr. Searles-Wood, jun., was led—simply from the description of the loess of Belgium—to suggest the identity of the latter with the Upper Glacial clay.*

Prof. Suess has found, in the basin of the Danube, some important evidence pointing to the Glacial age of the loess. He showed me pieces of green Hornblendic rock that had been broken from large erratic blocks found in the loess near Vienna. This rock is not known to occur in place nearer than the Wechsel, at the Styrian frontier, and he thinks it must have been carried on and dropped from ice floating over the water from which the loess was deposited. At Wurflach there is the terminal moraine of a glacier that once came down from the Schneberg. This moraine contains many scratched and polished blocks of limestone. Many of these have been carried across the Steinfeldt and deposited on the flanks of the range of hills near Pitten, where the local rocks are mica-schists. They lie at a height of about 1100 feet from the sea, and at a distance of 9 miles from the terminal moraine at Wurflach, which is the nearest point from which they can have been brought. They were noticed by M. Morlot, who did not know, however, from whence they had been derived. Prof. Suess traced them to their source, and thinks they must have been carried by floating ice.

The animal remains found in and below the loess also indicate its Glacial age. I have spoken of the gravels that underlie it and belong to an earlier period, in the basin of the Rhine. Some of these gravels appear to be of the same age as the Cromer Forest beds and the Durnten lignites, being characterised by the presence of *Rhinoceros etruscus*, Falc., which has been identified by M. Lartet with the *R. Merkii* of the German and Swiss geologists. The deposits containing this characteristic mammal are well developed at Mosbach, near Biebrich, in the valley of the Main; they consist of sands and gravels overlaid by the loess. Besides the *Rhinoceros etruscus* they contain bones of *Elephas antiquus*, *E. primigenius*, *Cervus tarandus*, *C. megaceros*, and *Hippopotamus major*—a curious mixture of northern species with those that are supposed to have had a more southern range. Probably

* Ann. of Nat. Hist., vol. xiii., p. 185.

the southern mammals came northward in summer and retreated to the south in winter. This explanation of the mingling of the remains of northern and southern mammals in the same deposit was first given by Sir Charles Lyell, and has been since advocated with great ability by Mr. Boyd Dawkins. That this is correct—and not that we have here the mixture of the remains of animals belonging to two periods, one warm and the other cold, as urged by Mr. James Geikie—is evidenced by the land and fresh-water shells that accompany the remains of the Mammalia. These could not, like the southern mammals, retreat to the south on the approach of winter, and they are all of a northern character, fitted to withstand great cold. Dr. Sandberger informs us that many of them still live in the higher parts of the Main valley above Bamberg. Others, such as *Valvata naticina* and *Hyalina viridula*, are only now found in the north and north-east of Europe. *Pupa columella* is still living in the north of Russia, in Lapland, and on the Gemmi. *Patula solaria* is now found on the Eastern Alps and the Silesian hills, and others attain their southern limits at Frankfort.* From a consideration of these facts Dr. Sandberger comes to the conclusion that the climate must have been much colder than at present. It would appear to have been so if we take the evidence of the shells only, but when we add that of the mixture of the remains of northern and southern mammals we are rather led to the opinion held by Sir Charles Lyell and Mr. Boyd Dawkins,† that the winters were colder and the summers perhaps warmer than now; that, in fact, the climate was more continental, in consequence of the western coast of Europe having then embraced the whole of the British Isles and other lands now submerged below the sea. If, as has been advocated by Mr. James Geikie, the mingling together of the bones of the northern and southern mammals was not due to the range of their summer and winter migrations overlapping, but to the remains belonging to widely different periods having been afterwards mixed together,‡ it is inexplicable that mollusks denoting a warmer climate should not have accompanied the southern mammals in the valley of the Main. On the other hand, such a fact is fully explained if the winters were colder and the summers warmer, for the slow crawling mollusks could not retreat southward on the approach of winter along with the southern mammals,

* Geological Magazine, 1874, p. 220.
† *See* Cave Hunting, p. 397.
‡ Great Ice Age, p. 467.

and only those that could withstand the severe winters would survive.

The Dürnten lignites, which, like the Cromer Forest bed and the Mosbach beds, are characterised and have their geological horizon fixed by the presence of *Elephas antiquus* and *Rhinoceros etruscus*, have been shown by Prof. Heer—from a study of the flora preserved in them—to have certainly been formed in as cold and possibly a colder climate than the present. I may remark that Prof. Heer himself correlates the lignites with the Mosbach gravels and the Cromer pre-glacial beds;* and if the arguments that have been used to prove, from the existence of the former, that there have been interglacial warm periods, are of any weight, which I do not admit, then the glacial period that is supposed to have prevailed before the formation of the Dürnten lignites is one of which we have no traces in the British Islands, as it belongs to an epoch anterior to the growth of the Cromer Forest, which is older than any of our English glacial deposits.

Resting on the Mosbach sands and gravels, as well as on gravels of more recent age, and everywhere distinctly superimposed upon them, lies the loess. We have seen that in the gravels of more ancient date the climate was probably colder, or at any rate the winters were more severe than now. The fauna of the loess indicates a still colder climate. The hippopotamus, the southern elephant, and the southern rhinoceros are no more found. The mammoth abounds, and is now accompanied by the woolly rhinoceros. The reindeer and the Canadian elk are much more abundant than in the earlier period. Fresh-water shells occur but rarely, and belong to species such as *Lymnæus truncatalus* (the only one found near Wurzberg), which probably lived not in the water from which the loess was deposited, but in marshy spots above it. River shells are unknown: I have never seen even a fragment of one in the loess. The most abundant land shells are either those now ranging far to the north, or which, from their wide distribution, must be able to accommodate themselves to extremely varying circumstances. Thus the most characteristic of all the shells—the *Succinea oblonga*, which I have gathered in the loess of the Rhine, the Main, the Danube, and the Steppes of Southern Russia, and which Prof. Morris finds in the patches of loess preserved in fissures of the chalk near Maidstone—is, though widely ranging, essentially a northern

* The Primæval World of Switzerland (Eng. ed.), pp. 171 and 176.

species, being at present rare in the basin of the Rhine, but abounding much farther north, in Scandinavia and Northern Russia. The little *Pupa muscorum*, which nearly everywhere accompanies the last mollusk in the loess, ranges throughout Europe from Iceland and Lapland to the Mediterranean. The minute *Helix pulchella*, which I have also found very generally distributed in the loess, has a still wider range, being found from Siberia to Corsica, throughout Canada, and even in the Azores. *Helix hispida*, a common loess shell, and *H. sericea*, which replaces it near Wurzberg, range from Siberia to the Mediterranean.

The fauna of the loess is the same in the basins of the Danube and the Rhine, and in Southern Russia; it belongs entirely to the period of the mammoth and the woolly rhinoceros. Neither the highest nor the lowest patches of it contain the remains of any other fauna. It is one fitted to endure a cold climate. It is the culmination of a series showing a gradual refrigeration of the northern hemisphere from early tertiary times. The tropical forms of the Eocene strata are succeeded by the semi-tropical ones of the Miocene; these by the more temperate species of the Pliocene; then comes in the fauna of the Mosbach gravels, the Dürnten lignites, the Cromer Forest and the oldest Thames brick-earths, when palæolithic man first appears on the scene in Northern Europe, and when the mean temperature was probably about the same as now, though the winters were colder and the summers warmer. Then, distinctly superimposed upon the Mosbach sands and gravels, comes the last fauna of all before the present,—that which is found in the newer gravels and the loess resting on them,—the fauna of the mammoth and the woolly rhinoceros, along with which we find the latest remains of palæolithic man in Northern Europe. That that fauna belongs to the Glacial period is evidenced not only by the fact that it is composed of species best able to withstand extreme cold, but that in the valley of the Danube large boulders occur in the loess that must have been transported on icebergs whilst it was being deposited.

No more arctic fauna is known in the basins of the Danube and the Rhine than that of the loess. Above it, as elsewhere, at this stage, there is a great break in the succession of life; the mammoth, the woolly rhinoceros, and palæolithic man disappear; and after that interruption the next forms that appear are those of the recent period. Neolithic man now takes possession of the land, and with him comes a number of animals that still remain with us, with the exception of

the Irish elk in Europe and the mastodon in America, which have perished since the commencement of neolithic times.

I have now given all the most important facts that I know of respecting the position and distribution of the loess, and we may proceed to consider the theories that have been proposed to account for its origin. First of all we shall have to determine, as well as we may from the facts before us, whether the loess was deposited in previously formed valleys which were as deep or deeper than they are now, or that it was laid down in the process of the excavation of the valleys. The former view was that held by Sir Charles Lyell, who had studied the loess both of the Rhine and the Danube, and who in his "Antiquity of Man" has given —with the logical force and clearness that characterise all his writings—a most able summary of the facts that influenced his judgment. These are, briefly, that the loess encircles some of the modern cones of loose pumice and ashes of the Lower Eifel; that it rests on gravel near the bottoms of the present valleys; and that there is evidence that before it was deposited there existed steep slopes, as between Darmstadt and Heidelberg, where loess 200 feet thick and reaching up to 800 feet above the river is banked up against an old valley slope composed of granitic rocks.

Prof. Ramsay, on the contrary, favours the opinion that the loess of the Rhine has been deposited by the river, which, as it gradually lowered the level of the plain, left its finer *detritus* at various heights above it.* The memoir in which this opinion is expressed is an essay to prove that the valley of the Rhine has been excavated by the river,—an opinion not likely to be disputed by any geologist. The question of the deposition of the loess is only incidentally mentioned, and no explanation is offered of the many facts advanced by Dr. Hibbert, Sir Charles Lyell, and others, to prove that before such deposition commenced the valley had been excavated to at least its present depth. The authority of the Director of the Geological Survey of England is so great that it is necessary to insist here upon the cogency of the arguments that have been urged against the theory he espouses, and it is a pity that he has not met them or even alluded to them in his memoir. We have already seen that the loess rests on previously deposited beds of gravel. At Wurzberg this gravel lies at only a slight elevation above

* Quart. Journ. Geol. Soc., vol. xxx., p. 89.

the level of the river, and Dr. Sandberger informed me that it goes down to at least 50 feet below it. Here, then, the valley must have been excavated below its present depth to allow of the deposition of the gravel which preceded that of the loess. Near Basel, as I have shown in Fig. 1, the older gravel rests directly against the steep slopes of the rocks bounding the valley, proving again that the latter had been excavated long before the deposition of the loess commenced. At Mosbach the gravels, containing an older fauna than that of the loess, though above the level of the river, are yet several hundred feet below the altitude the loess attains to, and that the latter has all been deposited since, is proved by it everywhere containing a younger fauna. The Mosbach gravels belong to the time of *Rhinoceros etruscus*, the loess to that of *R. tichorinus*.

These facts are very strongly in favour of the pre-diluvial origin of the great valleys, and there are others still more difficult to explain on any other theory. As already mentioned, the loess extends far up the flanks of the recent volcanic cones of the Lower Eifel, and at Mahlsberg attains an elevation of 600 feet above the Rhine. The most ardent advocate of the theory that the loess has been left by the river whilst it ran at higher levels will not, I think, suggest that the volcanic cones have been carved out by it, and yet excepting on that supposition their theory falls to the ground. Another most serious objection to it is that it requires that the whole of the great valleys of the Danube and the Rhine have been excavated in one comparatively short period, namely, that of the mammoth and woolly rhinoceros. The upholders of the theory may say—"Not excavated, but re-excavated through the gravels and clays that were deposited in them in Miocene times;" but the loess extends far higher up the sides of the valleys than we have any evidence that the Miocene deposits ever reached to.

In the whole of the long Pliocene period it would appear that the land was above the level of the ocean, and we are to suppose, on the theory against which I am contending, that the rivers did not then wear down their channels, but waited for the mammoth period before they commenced doing so. Both in the valleys of the Danube and the Rhine the Pliocene epoch is unrepresented by fossil remains. I asked Prof. Suess what was the meaning of this great hiatus? and he replied that during Pliocene times the rivers were re-excavating their channels through the Miocene accumulations, and that periods of destruction are not periods of deposition in the same areas. At first, the new river

channels would probably be cut out with precipitous sides, and it was not until these sides had been sloped down by the action of rain that the loess was deposited.

Adding to the arguments already urged the fact that nowhere in the loess have any river shells been found, whilst they abound in the beds of all our large rivers, we have a case so strong against the theory, that, until its upholders offer some explanation of the facts arrayed against them, we may fairly refuse to receive it. And if we are forced to conclude that the loess has not been deposited by the rivers, and that the excavation of the valleys took place at an earlier time, we must believe that in some way or other these valleys were again filled with water up to the height to which the loess reaches. This is the conclusion of the Austrian geologists with regard to the valley of the Danube, and that of Sir Charles Lyell with regard to that of the Rhine also; and the next question we have to consider is whether the waters were pounded up through oscillations of the surface of the land, or in some other way.

The valleys of the Danube and the Rhine were in existence in early tertiary times, and we find two great accumulations of fossiliferous strata in them,—one miocene, the other post-tertiary. Both appear to be due, not to deposits from rivers wearing down their channels, but to interruptions to their drainage so that their channels were raised. The first of these interruptions is known to be due to the great volcanic disturbances during which the Alps were partly elevated; the second I hope to show was caused by the ice of the Glacial period blocking up the drainage of the continent.

I have already alluded to the transported boulders in the loess, and to its fauna, indicating its glacial age. Along the northern flanks of the Carpathians, the Scandinavian drift rises to heights of from 1000 to 1200 feet; considerably higher than the low passes that lead into the basin of the Danube. General Helmerson has done me the honour to inform me that the transported northern blocks in Russia also range up to heights of from 1000 to 1200 feet above the sea. As the Russian diluvial clay can be traced continuously up to and around the Carpathians into the basin of the Danube, we may reasonably conclude that the water over which the northern drift was floated was the same as that in which the loess was deposited.

But it will be urged that "the northern drift is a marine deposit." It is so stated in most works on geology; but excepting around the southern border of the Baltic, and just so far as, and no farther than, the Scandinavian glaciers

reached and carried up fragmentary shells from the arms of the sea they had crossed, the northern drift does not contain sea shells or any other marine organism. For thousands of square miles, south of the irregular line I have indicated, up to and around the Carpathians, the northern drift is spread out, and not a trace of marine life—not even a diatom—has been recorded from it, whilst at its base, between the Oder and the Elbe, fresh-water shells abound. To believe that Europe gradually sank down below the level of the sea until the latter had its shore line more than 1000 feet up the flanks of the mountains, and that it rose again without the sea leaving behind it any traces of life excepting fresh-water shells, is such an extreme hypothesis, and so contrary to all we know respecting the composition of existing sea-bottoms, that it is probable that its present acceptance is simply a survival from the time when there was no other way of explaining the existence of water up to such heights.

The usual explanation of the facts of the Glacial period is one continued appeal to the hypothesis of great oscillations of the earth's surface at that time. It may well be questioned whether geologists have not been too ready to call in the aid of these movements. There is much evidence to show that vast continental areas were never below the sea-level from the close of the Palæozoic period up to the end of the Tertiary period. Yet after this stability of surface over such an immense period of time no hesitation is felt, in the comparatively insignificant Glacial period, of sending the surface of the land thousands of feet higher, that ice may accumulate on the now low ranges, and thousands of feet lower, that icebergs may float over the submerged lands; and no difficulty is experienced in believing that it should finish its wonderful oscillations by regaining the level it had before the Glacial period commenced. It seems a burlesque on Science that such theories should be prevalent amongst our geologists, and if they were not held by philosophers they would be ridiculed as unphilosophical. Those who advocate the former existence of these oscillations of the surface are those who urge that we should not call in the aid of any but existing agencies; yet where do they now find a shore-less and a shell-less sea? Put down a dredge anywhere in the ocean within depths of less than 2000 feet, and in the small quantity of clay, mud, sand, or gravel scraped up, it will be scarcely possible to take out a tea-cupful that shall not teem with marine organisms; yet we are taught that an immense area in Europe and America has been a sea-bottom, and every part of it a sea-beach as

the land rose again, without any evidence of marine life having been left behind.

But is there really no other way of getting water up to the heights we require without resorting to this extreme hypothesis? I have in former papers urged that there is—that the ice of the Glacial period flowed principally down the ocean depressions and blocked up the drainage of the continents as far as it extended, causing immense lakes of fresh or brackish water. I was first led to believe that the ice had effected this in studying the glaciation of North America, and in 1866 I advanced the opinion that the drainage of the St. Lawrence had been blocked up by the ice moving down from the north, and that thus a great inland fresh-water sea had been formed, over which icebergs floated.* I next, in 1874, applied the theory to explain the formation of the Steppes of Siberia, after I had crossed them and found that they contained fresh-water shells, and I further suggested that the ice that descended from the mountains of Scandinavia must have dammed back all the rivers of Northern Europe.† Mr. Croll informs us‡ that before this Prof. Geikie had suggested to him that if the Straits of Dover were not then cut through, or were they blocked up by land ice, "say by the great Baltic glacier crossing over from Denmark, the consequence would be that the waters of the Rhine and Elbe would be dammed back and would inundate all the low-lying tracts of country to the south; and this might account for the extraordinary extension of the loess in the basin of the Rhine, and in Belgium and the north of France."

Very soon after my paper was published in the "Transactions of the Geological Society" I became dissatisfied with the explanation I had offered, because I found that in Devonshire there were drift pebbles and transported boulders up to heights of 1200 feet above the sea, and also that the ice of Scandinavia had actually retired before the boulder clay was spread out: as in Sweden, the latter overlies the glaciated rocks and the till left by the land ice. This is also the case on the north-east coast of England, as shown several years ago by Mr. Richard Howse. The Scandinavian drift of the coasts of Durham and Northumberland was deposited after the land ice, that had scored and grooved the rocks below, had melted back, and when the land was

* Trans. Nova Scotian Institute of Natural Science, 1866, p. 91.
† Quart. Journ. Geol. Soc., 1874, p. 490.
‡ Climate and Time, p. 452.

covered with water over which icebergs floated, carrying the erratic blocks.

During a visit I made to North America, in 1874, I found many more proofs that the drainage of the north-eastern part of that continent was blocked up by ice that flowed down the bed of the Atlantic from the direction of Greenland, and I learnt from Prof. James Hall that Cape Cod was a huge terminal moraine. Knowing, also, that on the European side of the Atlantic Mr. Robert Chambers had been led to the conclusion that Scotland had been glaciated from the north-west, that Mr. Thos. Jamieson had shown that Caithness had been overflown from the same direction, and that there was much other evidence pointing to the same conclusion,* I was led to believe that the drainage of Europe had been blocked up—not by Scandinavian ice, but by that which occupied the bed of the Atlantic and had reached to our western shores.

If we try to picture in imagination what would happen if the Glacial period were to return again, the continents keeping their present form, we shall not find it so improbable as it seems at first sight that the ice should advance down the ocean beds and be thicker there than on the land. The first effect would probably be a great increase of the ice on areas such as Scandinavia and the Alps, where it exists at present, but especially on Greenland, which lies at the northern end of the great evaporating basin of the Atlantic. For the ice would accumulate fastest where the frozen precipitation was greatest, and without great evaporation there could not be great precipitation. We need not carry on in imagination what happened in Scandinavia and the Alps, for we know what did occur, and can trace the great extension of the ice by the marks it has left on the rocks it passed over. We need only, for the present, try to follow the progress of the far greater accumulation of ice that was taking place in Greenland. When it there reached to such a height that it intercepted all the moisture of the currents of air travelling over it northwards, the precipitation would be confined to its northern slope, just as the Atlantic slopes of Central America drain the north-east trades of their moisture, so that they are dry winds on the Pacific side. But as the precipitation would not, as in Central America, run down immediately to the sea, but be retained on the southern slope of the frozen mass in the shape of snow and

* See Quart. Journ. Geol. Soc., vol. xxxii., p. 85, where a portion of the evidence I laid before the Society has been published.

ice, the effect must have been that that southern slope would advance southward; so that the ice would progress not only by flowing southward as a glacier, but by the area of frozen precipitation being moved southward. Let us suppose that a ridge of ice 6000 feet high, on Greenland, would intercept all the moisture of the southern winds; then, if the precipitation every year on its southern slope was greater than what it lost by melting and evaporation, it must have advanced southward, and no depth of the ocean bed would prevent it doing so, so long as the excess of frozen precipitation continued.

Some geologists urge as an objection to the theory of the great advance of circumpolar ice, that it cannot be pushed up a slope by a force acting from a distance. In the words of the most eminent of these, the Duke of Argyll, "the theory assumes that masses of ice lying upon the surface of the earth more than mountain deep would have a proper motion of its own, capable of overcoming the friction not only of rough level surfaces, but even of the steepest gradients, for which motion no adequate cause has been assigned, and which has never been proved to be consistent with the physical properties of the materials on which it is supposed to have acted."* If this was a fair statement of the views of glacialists a very strong case would be made out against them, for it is evident to any one having a moderate knowledge of mechanics and of the physical properties of ice that it could not be moved in such a manner; and Prof. James Forbes proved many years ago that, even when flowing down the steep slopes of the Alps, the bottom layers of the ice are so retarded by the friction of the rocks beneath them that the upper ones slip over them, producing the structure he named "frontal dip." But it is not a fair statement of their views. Mr. Thos. Jamieson, one of the greatest authorities on the glaciation of Scotland by land ice, showed, in 1865, that if snow was heaped up it would spread out at the base, and flow off "not so much on account of the inclination of the bed on which it rested as owing to the internal pressure exerted by the immense accumulation of snow,"† and he further compared its movement to that of a heap of grain which flows off when poured down on the floor of a granary, the grains flowing over each other and spreading out with a very small pitch or slope. Prof. Dana, too, has entered very fully into the question of the

* Nature, vol. xiv., p. 436. Report of Address to Brit. Assoc. at Glasgow.
† Quart. Journ. Geol. Soc., vol. xxi., p. 166.

motion of land ice, and has shown that no slope of the land beneath is necessary so long as there is a slope of the upper surface of the ice itself down which it moves, and he has likened its motion to that of heaped-up pitch—a simile that Prof. James Forbes had also used.* There may be some geologists who believe that ice is pushed along as a solid, but certainly no such opinion has been advocated by leading glacialists, who only claim that if ice be heaped up it will flow outwards. The margin of the ice would, however, be pushed forward by the ice behind and above it in summer, when its fluidity was increased, just as in a railway-cutting we often see, in wet weather, clay slip down and push up the earth at its base.

In my theory I assume that when ice in Greenland was heaped up high enough to intercept all the moisture of the winds blowing northwards, the snow would be heaped up on the southern slope, and that, if the frozen precipitation exceeded the liquefaction and evaporation, the icy mass would move southward, and the highest portion of its crest would reach to lower and lower latitudes. It would be a ridge of ice and snow gathering to itself the moisture of the atmosphere, its very growth supplying a basis for future extension, like an invading army gathering its supplies from the country it was adding to its domain. From this ridge, as from a mountain chain, the ice would flow down, chilling the atmosphere as far as it reached, and carrying with it its own wintry climate. There are tree-less districts in America where there is not sufficient rain to support forest life. Yet if a small extent of country be planted, and sustained until the trees grow up, they will cause a greater rainfall. For the rain that does fall will not be allowed to run off the land to the sea, but much of it will be retained and returned again to the atmosphere by evaporation ; and as the amount of rain depends primarily upon the amount of moisture in the air, precipitation must be increased by increased evaporation. Thus a small forest may cause sufficient moisture to fall and to be retained, so as gradually to increase its limits and change a nearly tree-less country into one covered with forests. And so at the present time the ice is prevented from advancing into the temperate regions by a nice balance between the amount of frozen precipitation and of liquefaction ; but if that balance were only a little shaken in favour of the accumulation of the ice, it would obtain powers of

* *See* Dana's Manual of Geology, p. 536, for an excellent account of the motion of land ice.

self-sustenance and growth that might cause it to spread like a living organism.

The height of the ridge of ice and snow would increase as it advanced southward, for it would depend upon that to which the moisture would be raised before it was precipitated, and this would be greater in low latitudes than in high.

I believe it is essential in any explanation of the facts of the Glacial period to bear in mind that from its commencement to its culmination the zone of frozen precipitation was gradually advanced southward in the northern hemisphere. On the Continent of Europe it was moved from Scandinavia and the Swiss Alps to the Pyrenees and the Maritime Alps, and when the ice was highest on the latter ranges that on the former must have shrunk back on account of the precipitation being intercepted further south, just as there are no glaciers now on the Altai Mountains because the moisture is intercepted by the Himalayas. As the ice gathered on the Maritime Alps, the Pyrenees, and probably on the Cantabian Range, the glacial ridge of the Atlantic was gradually advancing southward, and at last I suppose it coalesced with the ice of the Pyrenees or of the Cantabian Range. I thought, up to a recent period, that the Atlantic ice might have reached the coast of Europe near Brest, and so blocked up the drainage of the northern part of the continent, but I obtained proof during a visit to the valley of the Rhone this year, that it also was included within the area of the Great European Lake. This would be effected by the ice of the Atlantic glacier and that of the Pyrenees or of the Cantabian Range meeting on one side, and that of Pyrenees and of the western prolongation of the Maritime Alps on the other. Or the ice on the Cevennes may have met that of the western Maritime Alps and that of the Pyrenees; or the ice of the Atlantic glacier may have flowed across the low stretch of country north of the Pyrenees. How, precisely, this gap was blocked up I do not yet know, as I have not been able to examine it for myself, and I have had to work out the whole problem of the advance of the Atlantic ice and its interception of the European drainage, alone and unaided.

The Atlantic ice has left traces of its progress on every island and coast of the western side of Scotland and Ireland. The glaciers of the Pyrenees have been traced down unto the low plains to the north of that range,* and with regard to those of the Maritime Alps Mr.

* C. MARTINS, Revue de deux Mondes, 1867.

Moggridge states that he has found abundant proofs that they descended to the level of the present Mediterranean, near Mentone.*

If, at the time the Atlantic ice and that of the southern ranges I have mentioned blocked up the drainage of Europe, the upper end of the Northern Pacific was also filled with ice, or only so far as the mountains of Kamtchatska, a great continental basin of water would be formed, rimmed in on its southern side by the ranges of mountains that extend from the extreme north-east of Asia to the Alps. The only break now existing in that rim is through the Bosphorus, and both physical geographers and naturalists have come to the conclusion, on independent grounds, that the Black Sea did not communicate with the Mediterranean until after the close of the Tertiary period, and the excavation of the channel that connects them must have been effected about the time of the Glacial period. Probably it was made when the whole drainage of northern Europe and Asia was intercepted and turned in that direction. That the waters of the Great Lake flowed that way is indicated by the Scandinavian drift having been carried far over the plains of Russia, and by the vast extent of diluvial clay spread out around the north-western shores of the Black Sea.

In this way I consider the waters were raised, over which floated icebergs from the north carrying the Scandinavian drift, and into this great lake the Danube and the Rhine, or the upper portions of them above its level, brought down fine mud from the glacier-capped Alps, which was deposited as loess. The waters everywhere were muddy, for glaciers were still triturating the mountains of Scandinavia, and thus the fine clay was spread over Europe as far as the lake extended. The very fact that the glacial waters carried fine mud in suspension to such great distances is another proof that they were fresh, for it has been abundantly proved by Dr. Sterry Hunt† and others that in salt water mud is quickly precipitated, whilst in fresh it remains a long time in suspension.

I think there is evidence that the lake reached a height of 1700 feet above the sea, and that it remained for a long time at about 1200 feet. It was once completely drained; at first gradually, but from about 500 feet above the present level of the sea suddenly and tumultuously by the breaking away of the icy barrier, and thus was produced a great deluge or debacle that swept over the lower lands and

* Proc. Geol. Soc., London, 1876, p. 127.
† Proc. Boston Soc. Nat. Hist., February, 1874.

covered them with a mantle of false-bedded sands and gravel. The evidences of this debacle were insisted upon by the last generation of geologists, and they are to be found everywhere over the south of England; but of late years they have been almost ignored, Prof. Prestwich being, I think, the only one who still advocates the old theory.

After being thus broken, the icy barrier soon closed up again, and the great lake was reformed, and this time was much more permanent, and probably existed until the communication between the Black Sea and the Mediterranean was cut through.

The areas with which this paper has had especially to deal are at an altitude of more than 500 feet, and no traces of the debacle could be expected, though I think there is evidence of the more gradual lowering of the upper part of the first lake in the separation of the loess into two divisions, as at Wurzberg. It is to the first rising of the waters that I attribute the destruction of the mammoth and the woolly rhinoceros, and probably of palæolithic man in Europe. The evidence is perhaps not so conclusive with regard to palæolithic man, but as concerns the two great quadrupeds it is clear and decisive. I can find nowhere in Europe a trace of their existence after the first rise of the waters. In the great debacle their bones were carried and spread out over the low grounds along with the lowland gravel, and doubtless often carried unto the top of low-lying patches of boulder clay, but in these cases they are broken, single, or rolled. And in the valley of the Rhone, as the great advance of the Alpine glaciers preceded by a long time the culmination of the ice of the Atlantic and Pyrenees, and the destruction of the great mammals did not take place until the approach of the latter event, there are apparent proofs of their post-glacial existence, but the evidence only shows that they lived after the culmination of the Alpine ice, not after that of the Glacial period. The evidence of the latter is not the glaciation of the rocks of the Rhone above Lyons, but the gravels and clays spread out in the same area by the great lake when there was a high ridge of ice to the southward and that of the Alps had shrunk back.

Probably palæolithic man, the mammoth, and the woolly rhinoceros survived through the culmination of the Glacial period in Asia, though the two quadrupeds soon afterwards became extinct. Mr. Boyd Dawkins and Sir John Lubbock have given cogent reasons for supposing that the Eskimos are the descendants of palæolithic man, and they still survive in north-eastern Asia. I found evidence when I crossed

Siberia in 1873 that the high lands to the north of the Altai had not borne glaciers even in the Glacial period, and they would thus afford a refuge from the great flood. But in western Europe most of the land that was not submerged was covered with ice, and there were few places of safety to flee to. What few there were, were in the east of Europe, and therefore it is, I think, that after the Glacial period passed away the animals that survived mostly spread from the south-east across Europe again, and only a few had reached England, and still fewer Ireland, before the waters of the ocean resumed their old channels, and cut off the communication with the Continent.

After the Glacial period, neolithic man, who had probably lived to the south and south-east for ages before that time, found central and northern Europe open to him. Not only the mammoth and the woolly rhinoceros had been destroyed, but, what was of much greater importance to him, the great Carnivora—the cave bear, the lion, and the tiger. The whole of northern Europe had also been covered by the fertile mud deposited from the Great Lake, and thus in every way the conditions of existence had been made more suitable for him.

The Glacial period is thus invested with a double interest; it is the first step backward in geology, the first forward in archæology, and in neither sciences can we make our footing secure until we clear away the doubts that beset us here. I claim for my theory that it shirks none of the difficulties and embraces all the facts. I have only dealt with a few of the problems in this and other papers, but I hope to find time to show in future communications that the theory affords a key to all. I can only trust that through time the faith of geologists in great upheavals and depressions of the earth's surface within a comparatively short period, in the possibility of the ocean having covered vast continental areas and retired without leaving any marine remains behind it, and in a succession of glacial periods having alternated with inter-glacial warm ones, will be shaken, and that they may look more favourably upon a theory that explains all the phenomena by one great advance southwards of the ice of a single glacial period.

VI. PHYSIOLOGY AND ITS CHEMISTRY AT HOME AND ABROAD.

By Chas. Thos. Kingzett and Henry Wilson Hake.

THE acquisition of any new and important fact in any branch of Science, in affecting more immediately that special branch whose pursuit has led to its discovery, exerts a further influence throughout all Science, which, in its gradual but universal action, may not inaptly be compared to the unlimited progress of a concentric wave. Not a few sciences owe their origin to the discovery of a single fact. Thus the discovery of Frauenhofer's lines gave rise to the great Science of Celestial Chemistry. In similar wise the Chemistry of the Carbon Compounds dates from Liebig's elaboration of the methods of organic analysis. So great was the impulse communicated by this perfection of method that from darkness it has led us to darkness again; for, in teaching us the composition of bodies, it has brought us to isomerism. When a new method of investigation shall impart a new impulse we shall again emerge into light. The influence thus originating with Liebig did not extend alone to pure chemistry; at the same time an impetus was given to Chemical Physiology, so brilliantly exampled by the work of Liebig himself. Progress has not, however, in the case of this Science been nearly as rapid as in the sister Science, and for obvious reasons. While Chemistry itself was by no means a new science, Physiology scarcely merited its name. Up to the time of which we are speaking physiologists had concerned themselves only with the study of anatomy and life functions, so far as these could be observed and explained without the aid of chemical science. Physiology, then, was at that period a science of observation and deductive reasoning, while metaphysical speculations entered largely into its narrow sphere. Liebig perceived that it was futile to appeal to the preconceived opinions of his contemporaries, and as regarded their metaphysical views he argued that such must render men powerless in time to perceive the relations of cause and effect. He devoted himself with much earnestness of purpose to expose the unscientific methods of reasoning and research then so prevalent among physiologists; he therefore addressed himself to a younger generation, and strove to impress their more plastic minds

with the principles of that inductive method of reasoning so ably interpreted by J. S. Mill, and of which he was so professed an admirer. Thus he gathered around him a group of men who, by adopting and disseminating his teachings, gave rise by their labours to a new epoch in Physiology and its Chemistry. And this brings us to the subject of our Essay. Let us put ourselves at once *en rapport* with the reader as regards our immediate object in writing it. Since the time above alluded to, while opportunities for advancement have been daily increasing, the study of Chemical Physiology has, in many respects, deteriorated. There is a necessity for reform. A reference to a few researches in Physiological Chemistry, published in certain leading scientific journals, will serve to substantiate this statement. We will commence by considering the alleged discovery of a new body, to which has been given the name of "nuclein." We shall enter into a minute account of this body, because its history is typical of a hundred others of a like kind. Its first appearance was in 1871, in which year a paper by F. Miescher* appeared in "Hoppe-Seyler's Med. Chem. Untersuchungen." It was regarded as a peculiar albuminous body, rich in phosphorus, and as constituting the principal component of the nuclei in pus-cells. Lubavin* next obtained an "entirely similar body" by long-continued digestion of milk-casein with artificial gastric juice. Dr. P. Plosz* states to have obtained the same body from the blood-corpuscles of birds and snakes, and also from brain albumen. Hoppe-Seyler* obtained, further, a substance from yeast-cells which he compares to "nuclein." Ignoring details, for the moment, "nuclein" is said to be obtained from these various matters after they have been freed from substances soluble in water, dilute acids, alcohol, ether, &c., by a process of artificial digestion with dilute hydrochloric acid and pepsin. The body in question is soluble in alkaline solutions, and has, as admitted even by those who have worked with it, the further general characters of a form of albumen. Thus it gives a xantho-proteic reaction with nitric acid, and its soda solution gives no reactions which can be said to be characteristic of a new substance. From this it will be seen, the only evidence that the body is at all new to science consists in its contained phosphorus. More recently† Dr. Miescher, of Basel, asserts once more to have obtained this "nuclein" accompanying "protamin" (of which more anon)

* Maly, Jahresbericht der Thier-Chemie für 1874. Wiesbaden, 1875.
† *Ibid*, p. 337.

in the spermatozoa of some vertebrates. Finally, its presence in human brain is re-asserted by Rudolf von Jaksch,* of St. Petersburg, a student in Hoppe-Seyler's laboratory. We here reproduce, in tabular form, the percentage composition of these various preparations, as given by the several authors :—

	From Yeast-Cells, by Hoppe-Seyler.	From Milk-Casein, by Lubavin.	From Pus-Cells, by Hoppe-Seyler.	Pure Nuclein from Spermatozoa, by F. Meischer.	From Brain, by Jaksch.		Analysis given in Hofmann's "Lehrbuch der Zoo-chemie."
C =	43·00	48·50	49·58	36·11	50·60	50·50	49·60
H =	6·06	7·15	7·10	5·15	7·40	7·80	7·00
N =	15·31	13·30	15·02	13·09	13·12	13·15	14·00
P =	2·58	4·60	2·28	9·59	2·08	1·71	2·50
O =	—	26·45	—	36·06	—	—	25·10
S =	—	—	—	—	—	—	1·80

Some of these authors write of a modification of "nuclein," insoluble in alkalies, in addition to the above. The following table shows the percentage composition of some forms of albumen :—

	Albumen.	Mucin.	Gelatin.	Fibrin.	Syntonin.	Lardacein.
C	53·5	52·4	50·16	52·7	54·1	53·6
H	7·0	7·0	6·60	6·9	7·2	7·0
N	15·5	12·8	18·30	15·4	16·1	15·0
S	1·6	Nil.	0·14	1·2	1·1	1·3
O	22·0	27·8	24·80	23·5	21·5	23·1

If these figures be studied, side by side, with those attributed to "nuclein," it will be seen that the percentages of hydrogen, nitrogen, and sulphur (where it occurs) in "nuclein" are precisely those which belong to various, and not unlikely impure, forms of albumen. Further, the phosphorus ranges in "nuclein" between 1·71 and 9·59 per cent, while the carbon ranges between 36·11 and 50·60 per cent. As we shall proceed to further demonstrate that this "nuclein" is a form of albumen, we may at once explain the absence of sulphur in most of the preparations by the fact that the authors heated them with caustic soda or potash, which would remove the sulphur from many forms of albumen : in fact this constitutes the method of preparation of alkali albuminate. As for the phosphorus, we contend that its presence proves but one thing, viz., the impurity of each of the above preparations containing it. How it exists there is also readily explained. Lecithin is a known constituent

* Pflüger's Archiv., vol. xiii., p. 469.

of each of the matters from which "nuclein" has been derived; and confining our attention to the human brain, this may serve to illustrate all cases. Jaksch directs the brain to be skinned, washed with water, passed through a sausage-machine, then to be extracted successively with cold alcohol and cold ether; after which it is to be pressed, rubbed, and extracted with absolute alcohol, and filtered. The residue is then to be digested with extract of pig's stomach and dilute hydrochloric acid during forty-eight hours, at 40° C.; the residue is extracted with soda, and the amber-coloured solution yields "nuclein" on precipitation with hydrochloric acid, accompanied, as the author states, by the evolution of a most peculiar odour, which he suggests may be sulphuretted hydrogen! Without claiming absolute purity for his preparation, he entertains no doubt "that it is a nuclein kind of body in a Miescher" (why not "Pickwickian?") "sense."

Now, in considering a little more closely the properties of this substance, we find that before it will give the phosphate test with molybdic acid it has to be boiled with stronger acid. Our acquaintance with brain-matter guarantees us personally in saying that Jacksch's proceedings would leave behind in the brain residue very considerable traces of myeline, and perhaps lecithin. Without pausing to explain why this is, we know that any phosphorus left behind would, on boiling with an acid solution, be yielded as glycerophosphoric acid, and this with molybdic acid would give the reaction which so astonished Jaksch. We tarry only to characterise one other statement made by Jaksch, which may be given in his own words:—"It will, however, be easy to estimate quantitatively, as nearly as possible, the amount of nuclein in the brain, from the standpoint that in the brain no other phosphorised principles occur except lecithin and nuclein. One requires only to remove the lecithin from the brain, and to subject the residue to a calcium and phosphorus determination."

The fact that the above was printed after the publication of Thudichum's researches in brain chemistry* intensifies rather than palliates the ignorance it displays. We are compelled to refer once more to F. Meischer. In a later paper† this author states the formula of nuclein, from the analysis above quoted, to be $C_{29}H_{49}N_9P_3O_{22}$, and opines that

* Med. Off. Rep. Privy Council and Local Government Board, New Series, No. III., p. 113.

† Jahresbericht der Thier-Chemie (MALY) für 1874. Wiesbaden, 1875.

the body is a tetrabasic acid. Indeed he states that he has calculated these figures and formula from a barium salt, for which he only gives the percentage of barium. He further says that it may combine with sodium and "protamin" in various different ways, giving rise to many definite and new striking compounds. Accompanying the "nuclein" from Spermatozoa was a base (the above protamin) whose hydrochlorate was difficultly soluble in alcohol. From the analysis of some platinum salts he has calculated a formula for the free body (protamin) of $C_9H_{20}N_5O_2$, but in the specific analyses from which he has made these calculations he has not given the percentages of platinum and chlorine found. Taking, however, one of his analyses as follows—

C	=	23·160
H	=	4·355
N	=	15·000
O	=	6·252
Pt	=	24·643
Cl	=	26·590,

and calculating out these figures in the ordinary way (Pt = 1), the following empirical formula results :—

$$C_{15}H_{33}N_8O_3,2HCl,PtCl_4.$$

In this computation we have calculated his chlorine on the assumption that it is associated with the platinum in the relation 6 : 1 ; for in no analysis does he give determinations of all elements present in the molecule of his preparation. That his formula for protamin is therefore incorrect, on the surface of things, is proved by a mathematical consideration of the data furnished by the author himself. Piccard, in the same journal, shows that what Meischer really had in his hands was a mixture of sarkine and guanine, though this critic admits that beyond these substances there certainly was a new base. We would suggest for that "new" base the old name, which it has always borne, of lecithin or of neurine. We shall dismiss this subject with the remark that, as regards these "nuclein" and "protamin" precipitates, they may not unfitly be classified, together with certain human beings, under one comprehensive heading—the Great Unwashed.

More seriously we feel that, as a result to Science, whatever might have been valuable in these researches is, for the time being, absolutely valueless.

Another example of similar work is presented in a paper

by Arcadius Rajewski* (of St. Petersburg), "On the Occurrence of Alcohol in the Organism." The experiments were conducted in Hoppe-Seyler's laboratory. Alcohol was injected into the stomachs of rabbits, and after some hours the animals were killed, their brains removed, and these distilled with water, in order to ascertain whether a final rectified distillate would give Lieben's iodoform reaction: it did so. This result induced the author to repeat the experiment upon the brain of a rabbit to which no alcohol had been administered: a like result was obtained. Other experiments were now instituted with the muscle and livers of alcoholised and "sober" rabbits, with no difference in the obtained results. Again, large quantities of ox-flesh, horse-flesh, and the flesh and livers of dogs and horses, were similarly treated; in each case a fluid distillate being obtained which gave the iodoform reaction, &c., but "which would do anything but burn."

The conclusions drawn by the author from these experiments are as follows:—"That for the estimation of alcohol in the system Lieben's reaction is of no use, since in the animal organism there are portions existing which on distillation give alcohol; or the organs of animals contain an ever-preformed quantity of alcohol."

Now, it is well known that Lieben had himself found the reaction which bears his name to be of no use for detecting alcohol in the urine.

In further reference to this subject we would add that Dr. Percy, in 1839, published a research on the presence of alcohol in the ventricles of the brains of animals to which it had been administered, and concluded that a kind of affinity existed between the alcohol and the cerebral matter. Every other result of Rajewski's had been previously arrived at and published by Dupré and others, and more correctly interpreted. These observers showed conclusively that a substance exists in the animal body, and could be obtained therefrom, which gives many of the reactions of alcohol, but which is not alcohol. It is certainly desirable that experiments should be conducted with a view of ascertaining the extent to which alcohol may locate itself in the brain; but this is not to be done in the way Rajewski has attempted it. It may here be stated that one of us is at the present time occupied with this matter.†

Another typical research will suffice to illustrate a

* Pflüger's Archiven, xi., 122.

† Kingzett, "Alcohol on the Brain," Chemical News, xxxiv., p. 158.

sensational aspect of the inferior kind of work presented by the scientific literature of our day. The author, F. Salmi, professes, in a discursive paper* of great length, to have succeeded in isolating "an alkaloid occurring in the brain, the liver, and the wild poppy." He appears to have obtained a substance from each of these sources which may have been anything in each particular case, for it presents no reaction stamping it as a new individual. Nor does Signor Salmi give a single analysis of any kind.

In leaving for a time the consideration of misguided research, we turn for awhile to consider the educational literature of our subject, and this will be best done by reviewing in brief certain typical text-books of modern physiological chemistry.

In 1873, among other text-books published in this country, was a "Handbook for the Physiological Laboratory,"† by Dr. Klein, Prof. Burdon Sanderson, Dr. Michael Foster, and Dr. Lauder Brunton. The Report of the Medical Officer of the Privy Council, containing the Brain Chemistry, had certainly not been published when the Handbook appeared: still the literature of Brain Chemistry was, at that time even, sufficient to form the basis of an ordinary octavo volume; but in the 572 pages of the Handbook we find little more than one page is devoted to the chemistry of the brain. In that part of the Handbook devoted to the Chemistry of the Tissues, by Dr. L. Brunton, the student is directed to estimate the amount of water in brain-matter by drying it over sulphuric acid, and, further, to grind the brain to paste in a mortar. The former is, in our opinion, as impossible as the latter is unfeasible. Following the methods here laid down the student would not succeed in isolating a solitary brain principle in a pure state, while with regard to that one principle, viz., cerebrin, about which the instructions are most minute, operations are detailed for its purification which must inevitably result in its destruction. We are told that to obtain pure cerebrin it should be, among other things, boiled with baryta water. Now this proceeding splits up cerebrin into sugar, a new base, and certain other products. Moreover, cerebrin is not hygroscopic, as stated by Dr. L. Brunton. We take this opportunity of paying a tribute of respect to the valuable researches of W. Müller

* Gazzetta Chim. Ital., v., 398.

† Handbook for the Physiological Laboratory, by E. Klein, M.D.; J. Burdon Sanderson, M.D., F.R.S.; Michael Foster, M.A., M.D., F.R.S.; and T. Lauder Brunton, M.D, D.Sc. Edited by J. Burdon Sanderson. London: Churchill. 1873.

relating to the discovery of cerebrin.* As a matter of history it is interesting to recal how preference was given by his contemporaries, among them his own friend Von Gorup-Besanez, to the work of Liebreich,† and how since that time Liebreich's protogon has been proved to consist of a mixture of cerebrin and myelin,‡ while Müller's cerebrin remains a definite individual. Space alone prevents our entering here into observations regarding the extent to which the action of baryta upon cerebrin may have misled Müller, for with that action he was not acquainted. In those pages of the Handbook devoted to Saliva we meet with the following passage :—"When poured from one vessel to another," says Dr. L. Brunton, " it (saliva) is seen to be more or less viscid, in consequence of which it is generally filled with air-bubbles. If none are present they are readily produced by blowing into the liquid by a narrow glass tube, when it is seen they take a long time to subside." And somewhat later, referring to methods of stimulating the secretion of saliva, he recommends one to " collect a considerable quantity, by filling the mouth with ether ;" and again, " roll a pebble or a glass stopper in the mouth, and attempt to chew it." Another matter to which we take exception in this part of the book is that, while Thudichum has worked more than any other chemist living on the biliary colouring-matters, his name does not occur in connection with this subject ; indeed the only mention of his name throughout the book consists in a bare reference to his treatise on the " Pathology of the Urine." Dr. Brunton gives Städeler's formulæ for bilirubin ($C_{16}H_{18}N_2O_3$) and biliverdin ($C_{16}H_{20}N_2O_5$). These formulæ had been withdrawn by Städeler at the time when Dr. Brunton wrote, and he had substituted other formulæ which were theoretically constructed on a perversion of Thudichum's own results. The true formulæ for these substances are—for bilirubin $C_9H_9NO_2$, for biliverdin‖ $C_8H_9NO_2$. Kryptophanic acid, which was proved by Thudichum to constitute the normal free acid of human urine ($C_5H_9NO_5$), and whose compounds have been thoroughly worked out, is not even mentioned in the Handbook. On the other hand, indican ($C_{16}H_{12}N_2O_2$) is said to be ever present in urine. We have personally examined many different

* Ann. Chem. Pharm., 105, 361 (1858).

† *Ibid*, 134, 29.

‡ Med. Off. Rep. Privy Council, New Series, No. III., 1874.

‖ *See* " State of Animal Chemistry in Austria," by J. L. W. THUDICHUM, M.D. Chemical News, April 13th and 21st, 1876 ; and Manual of Chemical Physiology.

specimens of human urine, and find the above statement to be without foundation. Schunck, on whose researches this statement was originally based, obtained from indigoferæ a principle which on treatment with acids split up into indigo-blue on the one hand and sugar on the other. It was therefore supposed to be a glucoside which furnished these matters. Subsequently, by suitable treatment, he claimed to have demonstrated the presence of a similar substance in normal urine. To this substance was given the name of indican ; but its presence in urine at any time is extremely doubtful, for in the first place no such substance has ever been isolated from urine, and, in the second place, when that blue colour is obtained which is supposed to be characteristic of indigo-blue and referable to indican, side by side with it *sugar is not produced*. When a solution is obtained which exercises a reducing action on Fehling's copper solution, there is every reason to believe the case abnormal in some respect. When urine is treated with its own volume of hydrochloric acid, a blackish precipitate falls on standing, and this, after extraction with dilute acid and then with absolute alcohol, is said to leave indigo-blue upon the filter. We have rarely met with cases when any blue is obtained, and when it has been obtained it has amounted to little more than a stain. But it will be evident that in any case it is not justifiable to speak of indican as a never-failing constituent of urine. Further, the name should no longer be retained, for if indican be a glucoside, and if the blue colour from urine be unattended by sugar, then at least the substance in the urine cannot be indican.

It is pleasant to be able to speak in eulogistic terms of Dr. L. Brunton's clear and succinct account of the bodies known in chemistry as albuminous. Such a task is as difficult as it is unthankful, for arranged under this heading there are almost endless forms of matter which, having many characters in common, have such complicated molecular constitution that hitherto it has defied the attempts of all who have worked upon the subject, to construct a formula for any one of them which is entitled to absolute acceptance. But it will ultimately be found possible to assign such a general constitutional formula to these substances that, while it may characterise no one individual body, it shall yet include and represent all. In this direction the researches of Schützenberger on the albumens, and Sullivan in other directions, encourage us in the hope that the day is not far distant when this may be accomplished.

We are strengthened in this conviction by our increased

knowledge of the chemistry of the brain. Here occur groups of phosphorised principles which, for the time being at any rate, we may view in a parallel light. For many years we knew nothing of the form in which phosphorus exists in the brain, and, having passed through that stage of its history when its presence was explained by definition, Gobley discovered egg-lecithine, and proved that, by means of chemolytic agents, it was split up into glycerophosphoric acid, a base, and fatty acids. Next, Thudichum discovered other principles in the brain containing phosphorus, namely, kephalin and the forms of myelin; and on continued investigation it has transpired* that all these principles are constructed on the same type, and may be said to be glycerin either in its simple or condensed forms, in which hydroxyls are replaced by fatty acid radicles on the one hand and phosphoryl on the other; the phosphoryl in its turn having hydroxyl replaced by a nitrogenised basis radicle.

It is only in the possession of such knowledge that we are able to appreciate to the full the enormous difficulty experienced by investigators concerning like highly complicated molecules, for the difficulty in the instance we have given is increased through the previously unknown nature of some of the fatty acid radicles. Moreover, the fact that we are at length able to classify such groups of bodies is by no means the least proof of their chemical individuality, for it places in the hands of the worker a law which must be obeyed, if the body to which it is applied belongs to that class.

This brings us again to the albuminous bodies of which we were treating, and to a study of a line of research which —so far as it has yet extended—has led but to insignificant results. We refer to the action of pepsin upon albuminous substances. Of pepsin itself we may be said to know nothing, except that from time to time statements crop up in current literature purporting to the alleged discovery and isolation of the actual ferments contained in it. But no analyses are forthcoming, and these excitements are short-lived, and leave us once more in our old position. Yet it must be acknowledged that the researches of Erlenmeyer (in part published) and of Alex. Schmidt promise to throw much light upon these subjects. It is not surprising, then,

* THUDICHUM and KINGZETT, "On Glycero-phosphoric Acid and its Salts, as obtained from the Phosphorised Constituents of the Brain." Journ. Chem. Soc., Ser. 2, No. 163.

that our knowledge of the peptones is no greater. It seems to be the object of certain men to test the action of pepsin upon all sorts of albuminous bodies in acid solutions, in order that when they have obtained their products they may pass them through a certain number of tests. These tests may be said to consist of the precipitability by neutralisation, boiling, or by stronger acids, and by such reagents as potassic ferrocyanide and cupric sulphate. But while they recognise that these peptone solutions give precipitates with bodies like tannic acid, argentic nitrate, mercuric chloride, platinic chloride, and plumbic acetate, it seems never to occur to them that it would be more profitable to follow up any one of these operations, and by so doing obtain, at once and for ever, a definite principle by definite means. As it is, however, our literature and our text-books abound with descriptive matter concerning what may appropriately be termed "test-tube chemistry." The test-tube as an agent in pioneering experiments, as a preliminary to the application of elaborated methods of research, based, indeed, in the first instance, upon such reactions, is invaluable; but employed as a final test of character and constitution, these reactions are absolutely without meaning. A precipitate or a colour may result from the mixture of two solutions, but no amount of repetition of such reactions can teach us more than the experience of the fact. To the precipitate we must apply methods of purification, and analysis, in order to ascertain its composition, and for the colour we must use the spectroscope, or other means at our command which shall in like manner identify for us its nature. But when we have effected all this we have but begun, for it yet remains to learn the inner constitution of the body whose empirical formula we have established; even then we have yet to determine in what way it was in the first place produced from the mixture. Then, and not till then, are the resources of the chemist exhausted and the problem solved. Now, by some of the reactions above indicated, it will appear reasonable that phospho-molybdic acid would precipitate many peptone solutions, in which case it is not unlikely that by the subjection of the products to decomposition with baryta water, and further treatment of the free substances, some very definite results would be obtained. Meanwhile a general name represents all we know of substances which will one day prove to have a constitution, complicated certainly, but as evident and as well defined as those of the best-known bodies.

We now pass on to consider another text-book of physio-

logical chemistry,* by C. H. Ralfe. In this book, leucin (termed here a glycolamid)—

$$\left.\begin{matrix} C_2H_2O,HO \\ H \\ H \end{matrix}\right\} N,$$

is alluded to as a neutral body which does not form salts. Now it not only forms salts by combination with acids and bases, but it is moreover capable of expelling hydrochloric acid from cupric chloride : moreover, the formula the author quotes is itself explanatory of these facts. More startling, however, is the statement that the vegetable organism is "synthetical," while the animal "analytically reduces." Were this wholly true we should be at a loss to understand how it is that hematocrystalline exists in the blood, or the principles to which we have above alluded go to build up brain-matter. In speaking of a possible mixture of glucose and chlorides, the author directs the removal of the latter by argentic nitrate, and then says—"On concentration, the glucose will crystallise out." This account would have been more correct had he for the word "glucose" substituted "nitrates," and still more correct if—instead of using nitrate of silver—he had directed the use of oxide of silver. Why, too, he should class cholesterin ($C_{26}H_{44}O$, a monatomic alcohol) as a non-saponifiable fatty principle, or speak of the fixed oils as highly inflammable (meaning, of course, their products of decomposition by heat), we are at a loss to imagine. Again, he is in error when he says that most of the albumens contain sulphur and phosphorus. As for the method he gives for separation of oleic, palmitic, and stearic acids from a mixture of the same, we cannot do better than state it in a few words. He directs to saponify with potash in the presence of alcohol, and to decompose the mixture with sulphuric acid; the removal of the potassic sulphate thus formed, by filtration; and the concentration of the alcoholic filtrate. The palmitic and stearic acids are thus stated to crystallise out, while from the mother-liquor is obtained the oleic acid by distillation, extraction of the residue by ether, and evaporation of the extract. This, it will be seen, amounts to no separation at all, for the palmitic and stearic acids are still in mixture and unpurified, while the oleic acid obtained as described will contain also some palmitic and stearic acids.

* Outlines of Physiological Chemistry, by C. H. Ralfe, M.A., M.B. London: H. K. Lewis. 1873.

We are also told that uric acid never occurs free in normal urine, while damaluric and damolic acids are given as normal constituents; whereas uric acid is a normal constituent, and damaluric and damolic acids never occur in human urine, and only on one recorded occasion have these latter been obtained from the urine of a cow.

On the subject of the brain Mr. Ralfe reproduces that which has long since been disproved, viz., Fremy's conclusions* regarding the presence of so-called oleo-phosphoric acid, a body which received in Fremy's hands not a single complete analysis, and which was undoubtedly a mixture of other substances with lecithine. This is the more remarkable, for Fremy, in claiming the existence of this body, overlooked or ignored a far more important discovery by Couerbe, viz., his cephalote, which, however, Couerbe did not obtain pure; had he done so he would have obtained the kephalin of to-day. Unfortunately an accident induced Couerbe to assert the presence of sulphur in his substance, and on this account there was denied to him that credit to which he was so fairly entitled.

It is a pity that, in our acknowledged state of ignorance as regards the chemistry of the body, such analyses as the following should be given, of an organ regarding which, in the past, our information has been so imperfect :—

Cerebral Matter.

Water	80
Fats	5
Albumen	7
Extractives and Salts . .	8

But this analysis, given by C. H. Ralfe in his book, is no worse than many similar ones to be found elsewhere. Among these extractives and salts leucin and uric acid are erroneously included. The author falls into the same mistakes regarding bilirubin and biliverdin as Dr. L. Brunton, and alarms us by saying that the breath in certain diseases contains chloride of sodium, uric acid, and ammonium urates. In this book, also, as well as in the "Handbook for the Physiological Laboratory," the spectroscopical characters of hematin, so beautifully worked out by Stokes and others, are incorrectly described.

One more point, and we have done with Mr. Ralfe. He writes—"The inorganic substances occurring in the animal

* Ann. Chem., ii., 463; also Journ. de Pharm., xxvii., 453.

tissues and fluids are not chemically combined with the organic principles." The only exception he allows, in doubt, is alkali albuminate; we had always thought bones, above all things, established the fact that some at least of the inorganic substances are in rather strong combination with the organic principles, while, as a matter of fact, so strong is the combination between kephalin ($C_{42}H_{79}NPO_{13}$) and certain salts with which it is combined in brain-matter, by virtue of its alkaloidal constitution, that it can only be obtained in the pure state after the destruction of such combinations by hydrochloric acid, which has the power of uniting with it to form a hydrochlorate.

We now come to a treatise by Prof. Karl B. Hofmann, of Graz,* and we find that in the 144 pages forming the first published half of this book there is more space devoted to the consideration of "nuclein" than to the whole brain chemistry. Moreover, although Thudichum's researches on this subject have been published since March, 1875, he dismisses them with the following words :—"Thudichum's newly-published list of numerous compounds can here only find mention." In keeping with such imperfection, we are not surprised to find the same errors that we have already pointed out as present in previously mentioned works. To be just, we will point out that whenever an opportunity is presented of entering into an enunciation of the structural chemistry of commonly well-known substances, the author never fails to do so; as an instance of which eight pages are devoted to the structural formula of glycerin, a matter which is as far removed from any *advance* in physiological chemistry as any matter well can be.

That our readers may not deem us unreasonable in demanding for Thudichum's researches in brain chemistry that respect which they merit, we would point out that, while our position has personally associated us with this work, we are obviously precluded from entering in any detail into the subject. We would nevertheless explain that it has been conducted, during a period of five years, on such a scale and in such a manner as can only be secured by the outlay of money in the power of Government alone to provide. As a consequence, it stands out pre-eminently as one of the greatest researches in physiological chemistry of modern time, and has given such an impetus to this science as has not been felt since the days when Wöhler constructed urea,

* Lehrbuch der Zoochemie, von KARL B. HOFMANN, Prof. der Phys. Chem. au der Universität Graz (Wien, 1876).

and when Liebig gave to the science a new meaning and a new method. That this opinion will prevail ere long there can be no doubt; meanwhile it is the privilege of those who are more intimately acquainted with the work to assert their conscientious conviction of its value. It is indeed painful, after such reflections, to turn to a "Jahresbericht" of animal chemistry* for the year 1875, to find that the only notice afforded in these pages of Thudichum's work is taken from the "Centralblatt," which in its turn derived its knowledge from the "Chemical News." In the latter was published an abstract of a summary given orally at an ordinary meeting of the Chemical Society of London. It further appears that a communication had been made to Dr. Maly by Dr. Dreschfeld, of Manchester, whose name figures on the cover of this "Jahresbericht." Dr. Maly concludes his notice with these unworthy words: — "The critical remarks appended by Dreschfeld to his report, viz., that throughout impure smeary masses formed the bases for Thudichum's new substances and new names, cause the omission of so extensive an account to be a matter of little regret.—*M.*"

Leaving the immediate criticism of special research and current literature, we would dwell for a moment upon the accepted doctrine that the best work and the greatest number of results issue from German laboratories. Without being desirous of entering into a controversy on this matter, we wish to indicate that there are workers in England who appreciate to the full all that Liebig appreciated of the methods of science, and who are, moreover, capable of carrying out those methods. There is an universal lack of scientific work, and especially of research in physiological chemistry; but in any comparison between Germany and England, we are by no means disposed to concede the laurel to Germany. To those who are intimately acquainted with German scientific literature the fact is patent that the enormous mass of work published represents also Russian, Polish, Austrian, Hungarian, and Belgian, nay Italian, workers. It is also well known that as regards the large percentage of Russian workers, these are enabled, at the expense of their Government, to study science in Germany, and for the last twenty years all their work in physiology, chemistry, anatomy, and microscopy has been published in German periodicals in

* Jahresbericht über die Fortschritter der Thier-Chemie, von Dr. RICHARD MALY, Professor in Graz. Wiesbaden, 1876.

their own names. Confirmatory of our remarks we here give the following statistics:*—There were at Berlin at the beginning of the present year 1884 German students, including 162 owing no allegiance to the empire. The favourite university of the Germans is Leipzig, which has 2575 Imperial German students, of whom 939 are Saxons and 1143 Prussians. Third in rank, if judged only by the number of students, is Munich, with 1087 Imperial German students, of whom 912 are Bavarians and 113 Prussians. The distribution of students among the four principal faculties is as follows:—

	Berlin.	Leipzig.	Munich.
Theology	162	337	84
Jurisprudence . .	807	1130	257
Medicine	263	369	307
Philosophy . . .	911	1089	555

The difference between the sums of these columns and the above figures give us the number of foreigners not Germans studying in each university as follows:—Berlin, 259; Leipzig, 350; Munich, 116. Berlin has therefore a total of 2143 students; Leipzig, 2925; and Munich, 1203.

Now, if any comparison is to be instituted between German and English work, all these accessories on the former side must be deducted, or to English must be added American, Australian, and Indian work.

The inferior character of some of the German work seems to be referable, in part, to the system of their university teaching, for it is a fact that the professors of medical chemistry are often associated with applied chemistry; that is to say, their work lies between general chemistry on the one hand, and medical chemistry on the other. When it is therefore considered that the medical men are far behind Englishmen in their acquaintance with Materia Medica, and that these same men place in the hands of students work of a severe character in science, it is not surprising that the results should in many cases be so deplorable.

Let it be understood that this Essay is in reference to Physiological Chemistry, and that, in writing of certain untrustworthy work emanating from Germany, we have no wish to cast an imputation upon the mass of good work in

* Literattische Wochenbericht, of Leipzig, of February 24, 1876.

pure chemical science of which the nation is so justly proud.

We have advanced, in the foregoing pages, our conviction that in Physiology and its Chemistry there is a necessity for reform, and have substantiated our view by facts. We have shown that, here and there, men have lost sight of those ruling principles underlying all science, and have plunged blindly into the mazes of uncerebrated research, the conflicting results of which have reduced them to a compromise between their intellect and their honour. In pursuing research we must ever bear in mind the ultimate object of physiological chemistry, and the means by which this object may be attained.

Liebig assumed the existence of a vital force, powerful to develope from the seed the plant, and from the egg the bird; but he yet acknowledged, above all men, that the actual processes of life, with all their complications of function, were based upon the same laws which exist between matter and force in the chemist's laboratory. We cannot therefore but regard those who hold the opinion that there is something in life which renders negative all our conclusions drawn from laboratory experiments, and nullifies all our hypotheses of functions, as men who fail to grasp thoroughly the conditions upon which life depends.

The object of physiological chemistry, then, is the reduction to general laws of those phenomena which, in their multifarious co-relations, constitute the functions of life, in health and disease. To prosecute this study with hope of success it is essential that we should first become acquainted with the composition and constitution of those substances which are elaborated in the various tissues, organs, and fluids of the living body, and of which these are themselves constituted. It is only by means of such knowledge that we can be enabled to trace out the intricate concatenations of the various parts of the animal body, and of those metamorphoses which are constantly in process in the living laboratory.

From this it will be seen that the method in the study of Pathology is necessarily of the same order as that employed in Physiology, and the results of these parallel investigations appear as the expressions of health and disease.

In drawing towards the close of our Essay we admit that in the Science of Physiological Chemistry there are pending matters for research which might fairly be undertaken by men of ordinary attainments, at very little expense; but

that there are questions to be solved which are further removed from the abilities of the untrained mind, and which demand for their elucidation means at the command of but few private individuals, is, we may say, notorious and acknowledged. We have endeavoured, *en passant*, to indicate some of these problems, and did space allow it would be matter of interest to allude to many others. A few may conveniently be here given. In the first place, of the matters entering into the composition of so-called extractives we know extremely little: in fact, Liebig—who has worked at this qnestion more than any other man—was only successful in ascertaining the nature of less than 17 per cent of the total matter. Fortunately for science Liebig was more conscientious than many workers in this direction; for it is a weakness not only on the part of the physiological chemist, but on that of the ordinary chemist, in their investigations, to seize upon one particular product, preferably choosing the most crystalline, and to examine it to the neglect of the 99 per cent. This would not be of so much import if at the same time it were accompanied by a statement of the fact, but as it is, their publications are not rarely so worded as to convey to the mind of the reader, imperceptibly, the idea that nothing remains to be done. Again, with remarkably few exceptions, nothing is known regarding those relations which undoubtedly exist between the chemical constituents of the various organs, tissues, and juices of the body. We will refer for the moment, in illustration, to the identity* of the phosphorised principles accompanying blood corpuscles with one of the forms of myelin as existing in the brain. This discovery, however, is but the key-note to other discoveries which must and will be effected before we can even speculate on the functions of that matter itself, either in relation to the intellectual faculty or in regard to the blood function. That there are similar relations existing between other brain components and the bodies found in the liver and the urine, we have also no doubt, but the nature of these relations, or the connection of excretory products with the albuminous constituents of the body, is almost entirely hidden. But with the increase of our acquaintance with substances already known to Science, and with the discovery of greater numbers yet to be revealed, will come the explanation. The pursuit of this science,

* "On Hemine, Hematine, and a Phosphorised Substance contained in Blood Corpuscles." By J. L. W. Thudichum and C. T. Kingzett. Journ. Chem. Soc., New Series, No. 165, September, 1876.

like that of all pure science, is, it is well known, as unremunerative as it is laborious to those who undertake its investigations.

We conclude our Essay with the hope that our country —ever sensible of matters affecting the welfare of individuals and communities—may not neglect to nurture its institutions, nor forget those workers who are qualified to effect what yet remains to be done, as testified by that which they have already accomplished.

NOTICES OF BOOKS.

Aerial Navigation. By the late CHARLES BLACKFORD MANSFIELD, M.A. Edited by his Brother, R. B. MANSFIELD, M.A. London: Macmillan and Co. 1877.

THE late Mr. Mansfield was best known as an accomplished chemist, but he wrote upon various subjects, and possessed an uncommon breadth of culture. His "Travels in Paraguay" is a very suggestive book, quite beyond the usual run of narratives of travel. He was an accurate scientific worker, possessed a keen insight into the mysteries of Nature, and was moreover gifted with a fine imaginative faculty. He had something of the genius about him, and from whatever point of view we observe his faculties we see at a glance that he was no mediocrity. Some insight into the breadth of his reading may be obtained at the very outset. After the dedication of his "Aërial Navigation" "To the Industrious of All Nations," he places before his Preface a few quotations from various writers, which seem to strike the key-note of his theme: he quotes from Tennyson's "Locksley Hall," Jacob Behmen's "De Signatura Rerum," Friar Bacon's "De Secretis Operibus Artis et Naturæ," Chancellor Bacon's "Sylva Sylvarum," and Hans Christian Andersen's "Ugly Duckling." The work itself is unfortunately unfinished, and it is much to be regretted that no less than twenty-five years have elapsed since it was written; yet it is said that no book has appeared during that period which has taken the place in the literature of the subject which this volume was meant to occupy. It seems to have been thrown off from the author's brain, as a mere mental exploit, to relieve himself of a burden. "My object in writing it," he says, "will be simply to deliver my brain of a burden which came upon it uninvited."

The work is divided into three parts, of which the first states the problem to be solved, the second gives hints for its solution, while the third takes the form of some very useful appendices.

It is clear that there are two methods by which a body may be sustained in the air: first, by mechanical force applied to the air, as in the case of the flight of birds; secondly, by the attachment of a buoyant body, as in the case of a balloon. The author does not consider the sustention and propulsion of a man in the air, by his own muscular effort, an impossibility; and he further believes that a good grip of the air could not be obtained with a propelling surface and mechanism weighing less than 10 lbs., which we should have considered an extreme minimum.

A man in walking a mile has done work, as far as his legs are concerned, equal to that which would be required to raise him vertically through about 146 yards; and if we reckon that a sound man can walk 30 miles a day, he would expend sufficient force to raise himself and an apparatus of wings weighing 10 lbs. through a vertical height of $2\frac{1}{3}$ miles. If we suppose that his wings have each an area of 7 square feet, they must move through 18 feet in a second in order to neutralise the weight of the system, and any power above this could be used for raising and propelling the body. To remain poised in the air the wings must be kept vibrating with such a velocity that they move through $25\frac{1}{2}$ feet per second. We may suppose a man, without overtaxing his strength, to be able to make ten complete beats of his wings in a minute. The wings would have to be very gigantic: according to a calculation given on page 30 the distance to the centre of the wing must be 153 feet. The first essential condition of aërial navigation is stated to be—"That the propelling power must be so adjusted that it may be kept constantly horizontal, and that the force must be so applied that it shall have no tendency to make either gas-vessel or man-vessel deviate from a strictly horizontal position, and this whatever may be the speed of flight."—(P. 51.) The second condition is—"That the gas vessel must be kept perfectly stiff, so that it shall neither turn up nor be bent down at the ends, but shall keep its form under all varying conditions of load from below and of gas-charge within."—(P. 55.)

Many proposals have been made as to the shape of the gas-vessel. Sir George Cayley, in 1816, proposed that it should have the shape of a woodcock's body; others have proposed prolate spheroids and spheres; others a fish-like form. Our author concludes his chapter on the question of shape by the assertion—"The vessels of air-craft must be of an elongated form, to enable them, by cleaving the air and eluding its resistance, to receive the highest velocity attainable by the exertion of a given amount of power." The material must be impermeable to gases, and at the same time strong and light, and it must be charged with the lightest possible gas. Hence a cheap way of preparing large quantities of hydrogen must be found. The air-craft must retain its buoyancy unaltered: it must be able to retain both gas-vessel and man-vessel in a horizontal position, and to regain its level if disturbed. Then comes the great question of *power*. Numberless suggestions have been made on this score: steam, electro-magnetism, human muscular power, the traction of flying birds, &c. Then there must be appliances for transmitting to the outside atmosphere the force generated within the balloon, in such a way that there will be sufficient leverage to propel the craft forward. An interesting summary of the requisite conditions is given in the last chapter of the first part (pp. 174 to 182).

In the second part are various hints of great ingenuity and value for obtaining each of the requisite conditions. Unfortunately the work ends abruptly in the middle of the chapter on "Power." The headings, however, for continuing the chapter were found by themselves, and are given below :—

"Sources of mechanical power, of two kinds, may be considered—

(*a.*) Those which work on the spot.
(*b.*) Reservoirs of Force.

(*a.*) Divisible into—
1. Those that work by expansion.
2. ,, ,, contraction.

1 A. Heat applicable to—
a. Fluids.
β. Liquids.
γ. Solids.
a. Air Engines.
Benzole.—Explosive.
β. Steam.
Other Vapours.
Liquids which expand.
γ. Expansion of Metals.
1 B. Freezing Water.
1 C. Wood-absorbing Water.
1 D. Sodium and Water.
} Those that work by expansion.

2. *a.* Electro-magnetism.
β. Moistened Rope.
} By contraction.

(*b.*) Reservoirs of Force divisible into virtually Springs. To find the best spring :—
a. Mechanical.
1. Air will not do.
2. Carbonic Acid.
3. Nitrous Oxide.
4. Caoutchouc. Steel.
} Tides. Winds. Balloons. Gravity.
β. Chemical.
Gunpowder moistened.
Gun-cotton.
γ. Vital.
Human Muscle magnetised."

In the Appendix we find more than three pages of titles of books or memoirs consulted by the author in the composition of his work. From beginning to end of the book we are struck by the fact that the author has consulted every available source on the subject before committing himself to an opinion or offering a hint as to a more perfect method. The work must always be of value, if not for its suggestions, at least for the clear logical

manner in which the *pros* and *cons* are placed before the reader. But we claim more for it than this: we think the author has treated the subject in an eminently suggestive and often original manner, and we cordially recommend the book not only to the rapidly-increasing list of members of the Aëronautical Society, but to all readers interested in a subject which has puzzled mankind since the time of Dædalus.

A Classified English Vocabulary, being an attempt to facilitate a Knowledge of Words and their Meanings by an Arrangement of Ideas according to their Scientific Connection. London: Provost and Co.

This little book is absolutely anonymous, not bearing on its title-page even a *nom de plume*. From the Preface we gather that the fundamental conception of the work first suggested itself to the author whilst attending a lecture on Mnemonics, in which was illustrated the singular facility with which a long string of names could be remembered when every name was linked to the name which followed it by some observed connection between the ideas conveyed by them. We further learn that the construction of the classification adopted has been "very much facilitated by the guidance derived from the classification of ideas comprised in Dr. Roget's 'Thesaurus.'"

In attempting to form a conclusion on the merits of the treatise, a distinction must be drawn between the value of the author's fundamental conception and the manner in which it has been here carried out. We fully grant that not merely in daily life, but in public speaking, and even in writing, there is a great —and we fear an increasing—vagueness in the use of terms. Men do in fact "connect with the name no precise knowledge of the things, but apply it rightly or wrongly to every object which seems to possess this undefined sort of resemblance to various other things called by it." To give a familiar instance—Does not all England outside of strictly scientific circles persist in calling the cockroach a "black beetle," although it is no beetle at all, but a congener of the locust and the cricket? And, on the other hand, do not our American cousins call the new insect-scourge which is travelling eastwards from Colorado, and which is a true beetle, the "potato-bug?"

We fully agree with the author that such vague uses of language are a serious evil, and that any person by giving way to it tends to form "defective habits of observation" and a general looseness of thought.

But having admitted so much, the question remains—What is the remedy for this vagueness? On this point let us hear what

the author says in his Introduction :—"As signs serving to denote an indefinite number of resembling individuals, to identify species under genera, and genera of a lower grade under genera of a higher grade, the general names of a language may be considered not only as instruments of thought and of communication, but also as originally being in themselves the clue to an important body of knowledge. For every general name expresses certain attributes, and the similar things which it denotes are all things whatever which possess these attributes. In learning, therefore, to apply the name to various things possessing the attributes which it expresses, the learner will be at the same time gathering knowledge of the things themselves, as agreeing with one another and differing from other things in certain definite respects. In the case, therefore, of a well-developed language, or one embracing the whole universe of discovered or imagined things under various degrees of generality, its gradual acquirement by any one as his native tongue would appear to involve a vast accession of knowledge, a certain insight indeed into the nature of all things known. Attainment of this knowledge, moreover, would seem to involve as a result a mind made open to every variety of impression ; an intellect quick, through constant exercise, to observe, compare, discriminate, and identify,—to perceive differences between things which resemble, and points of agreement between things the most seemingly different from one another."

To us this passage seems fraught with dangerous error. To endeavour to acquire a knowledge of things by comparing, analysing, and classifying the names by which such things happen to be known, was a characteristic feature of the so-called "stationary period" of the human mind, and was one of the chief reasons of the impotence of the philosophies which prevailed before the epoch of Bacon, Descartes, and Galileo. No amount of inquiry into or speculation on words will enable us to discern minute differences between things which resemble each other, or occult points of approach between things seemingly distinct. For this purpose we must go to the things themselves. It is agreed by all competent judges that the mere reader, or the mere hearer, the man of words, however carefully he may weigh and define the terms which he uses or meets with, will have merely a dim and—so to speak—hearsay knowledge of the subject which he takes up. If we wish to know the properties of matter or of force, we must work with or upon it. That very vagueness of which our author justly complains is mainly due to the circumstance that in our ordinary educational training thing are left out of sight, and words are treated as if they were not counters, but money.*

If, to return to the illustration which we used, it is desired

* "Words are the counters of wise men, but the money of fools."

that the words "beetle" and "bug" should be used in an accurate manner, surely the best method is to point out to the pupil the distinctions between the two classes of insects denoted by these terms. If this is once done there will be no more mistakes of the same kind. We should also fear that our author's scheme, if used without the correction of a frequent appeal from words to the things which they represent, would rather tend to cherish the belief—already too common—in a "pigeon-hole" arrangement of the world. In physical science and in the affairs of daily life we have continually to classify; but our genera, groups, or whatever we may call them, are established not so much by reference to a definition, or outside boundary wall, as to a type within.

It must not be supposed, however, that we condemn the author's system in its totality. On the contrary, subject to the control of a constant appeal, whenever practicable, from words to things, from the symbol to the object symbolised, we think the general features of his plan might be advantageously introduced in vocabularies. Only let no exaggerated expectations be formed as to the nature or the kind of knowledge and of mental discipline that may be expected to result from its introduction.

As to the execution of the plan, we do not consider that the author's categories are altogether beyond the reach of criticism. Errors are not wanting. Let us, for instance, turn to "Class VI. Persons. Sub-class I. Persons in relation to Intellect." Here we find a distinction between "No. 369, *Thinker*, and No. 370, *Reasoner*," the former head being made to include the speculator, inquirer, examiner, investigator, explorer, censor, and critic. Now it is, we submit, impossible to speculate, examine, or criticise without reasoning, and hence we fail to see how the author's classification can be maintained. A little further we find "sceptic" introduced under the head "unbeliever." This may be in accordance with the conventional usage of the tea-tables and of "serious society," but in strictness a sceptic is one who neither affirms nor denies, but suspends judgment. Under the class "Dunce" figure the "sciolist" and the "smatterer." We wish the author had introduced the "good man all round" of modern examinations, who is incomparably the most hopeless specimen of duncedom ever produced.

Again, we find under "Class V. Things arbitrarily distinguished, constructed, or produced," Sub-class IV. Food. No. 331. Alcoholic liquors, ale, beer, porter, cider, and perry. But wine is found not along with these kindred fluids, but under "Class II. The material world. Sub-class VI. Organic nature. Section IV. Vegetable products. No. 132. Starch, sugar, and allied or derived substances." Why the juices of two fruits, pressed out and allowed to ferment, should be assigned respectively to two such distinct places in the author's classification we must confess ourselves unable to understand. Nor yet can we see why arrow-

root, sago, and tapioca should be banished from the class "Food." Now, a classification which admits of things respectively closely allied being placed under different heads must be rejected as essentially faulty.

Wine and its Counterfeits. By J. L. Denman. London: 20, Piccadilly.

The author of this interesting little pamphlet is well known for his advocacy of the merits of the pure wines of France, Hungary, and Greece, against the depraved public taste in favour of the mixtures known as port and sherry. Mr. Denman has done more than write; he has spent over ten years in making known the wines of the Greek Archipelago and Islands—a work, doubtless, far from profitable for the first few years, owing to having to fight against old and well-established prejudices.

The work contains several interesting tables relating to wine, valuable extracts from the researches of Dr. Thudichum, Dr. Dupré, Dr. Barry, Dr. Edward Smith, Dr. Faure, and many other eminent authorities.

The admirers of sherry will feel much comforted when they read the process by which their favourite wine is made, and how the wholesome tartrates and malates are converted into sulphate of potass by the operation of plastering: so successful is the result, and so rich in this salt are some samples of sherry, that it is rather a matter of surprise that it has not long ago taken its place in the Pharmacopœia as a purgative medicine.

Port is scarcely better: the finest samples may be very good as cordials, to be taken as cautiously as Curacoa and Maraschino, but they are certainly not wine, and should never be sold as such by any merchant who values his reputation as a dealer in pure and wholesome wine. There is not the slightest doubt that many of these mixtures come within the scope of the very unsatisfactory Adulteration Act, as fraudulent mixtures of a nature injurious to health. Gout especially is one of the diseases attributable to fine old port, a concoction for which the British nation has to thank that late eminent statesman William Pitt.

Wine analysis demands very much the attention of our chemists; some means of distinguishing between alcohol, the direct product of fermentation, and distilled alcohol is especially needed; they are two very different substances, varying greatly in their action, as anyone may try for himself by noting the effects of a glass of good claret, or other unfortified wine, and an equal quantity of brandy or whiskey diluted to a similar strength.

The chapter, "How to know Pure Wine," is not the least

valuable part of this little pamphlet, and is so useful that we quote it at length :—

"1. That pure sweet wine is of low alcoholic strength.

"2. That all perfectly fermented wine is dry, and of high alcoholic strength (varying according to circumstances, and then rarely exceeding 26 to 28 degrees of proof spirit), and is not sweet, as all the sugar from the grape has been converted into alcohol, and should have when young an acid, or rather sub-acid, taste (not acetous), from the presence of tartaric acid, which is the natural and healthful acid appertaining only to wine. Its removal, by gypsum or plaster of paris, converts the tartrate of potass into sulphate of potass, which is a purgative and bitter salt, with a depressing action on the heart. All sherries are plastered.

"3. That the addition of alcohol to wine, either before or after fermentation, renders it unwholesome, and conduces to gout and similar disorders.

"4. That the greater the amount of *natural* alcohol produced in wine the greater is the amount of body in it, as the other constituents of wine must have been produced *pari passu*, and have been existent in the grape to yield the amount of alcohol, whereas added spirit does not give body.

"5. That the greatest amount of natural alcohol in wine is produced in those climates in which the grape attains the greatest perfection, and consequently contains the largest amount of sugar, combined with the other constituents of the fruit.

"6. That as all port, sherry, Madeira, Marsala, Catalonian, and Roussillon contain from 36 to 42 per cent of spirit, they have been either checked in the fermentation by the addition of alcohol to retain the sweetness of the must, or, after the fermentation was completed, the wine must have been sweetened and spirited to bring it up to the regulation standard. Furthermore, that added spirit causes an undue deposition of the tartrates and neutral salts of the wine (thereby depriving the wine of that life, freshness, and character which render it so valuable as a remedial agent), covers defects, and enables all sorts of mixtures to be made up and sold as port, sherry, &c.

"7. That the addition of alcohol to wine renders it of less pecuniary value, as spirit costs about 1s. 8d. per proof gallon without the duty, which is much less than the cost of wine : it therefore follows that, if it were not for the Excise and Customs Duty, spirit, the strength of ordinary port and sherry, could be sold for 2d. per bottle.

"8. That all natural wines, if any improvement is to be effected by age, must throw down a deposit, and thereby become *sweeter in bottle, by the elimination of their tannin, tartrates, &c.* From red wine the deposit contains tannin, which, uniting with the albuminous matter contained in the wine, forms

a crust, that year by year becomes less and less, until at length it becomes so thin that it acquires the name of 'bee's wing.' The deposit also takes the form of crystals, which will both adhere to the cork and fall to the bottom of the bottle like powdered glass. All natural wines that have been any length of time in bottle should therefore be decanted with care."

Description Physique de la République Argentine. Par le Dr. H. Burmeister, Directeur du Museo Publico de Buenos-Ayres. Traduite de l'Allemand par E. Maupas. Tome Premier, contenant l'histoire de la découverte et la géographie du pays. Paris: F. Savy. 1876.

We have before us the first volume of an elaborate and important work, which Dr. Burmeister contemplates issuing on the Argentine Republic, and in which will be embodied the results of twenty years' careful study. It is intended that the first two volumes should form the introduction, as it were, to the main body of the work, the scope of which, as a whole, will be best and most concisely described in Dr. Burmeister's own words:—"Je n'ai pas à fournir," he remarks, "un traité de géographie de la république Argentine, et encore moins une description de sa richesse minéralogique; mais il me suffira de faire connaître dans leurs généralités le sol, et le milieu dans lequel, vivent ou ont vécu dans les temps préhistoriques les animaux et les plantes qui seront étudiés spécialement dans les volumes suivants. L'ouvrage sera surtout consacré à ces deux règnes, et son but est de donner un tableau des diversités organiques de ces deux groupes, tableau qui commencera par le règne animal. Le règne végétal et la description géologique du sol de la république ont étés confiés à de jeunes savants qui publieront leurs divers travaux sous leur nom personnel, comme parties de l'œuvre totale."

It is naturally impossible to form a decided opinion as to the merits of this work from this the first volume, but Dr. Burmeister's reputation as a geographer will be a good guarantee for the correctness of his facts and the soundness of the views expressed; and it will be sufficient for us to observe that the present instalment of the book is devoted to a history of the discovery and of the early colonisation of the country, and, secondly, to a sketch of the geography of the republic, in which the author has availed himself of special materials to be found only at Buenos Ayres. It may not be uninteresting to add that the work is published in German and French, at the expense of the Republic.

The Empire of Brazil at the Universal Exhibition of 1876 *in Philadelphia.* Rio de Janeiro: Typographia e Lithographia do Imperial Instituto Artistico. 1876.

The only works hitherto published which afford very complete information respecting the Empire of Brazil are the "Breve Noticia" and "O Imperio do Brazil," prepared for the Paris and Vienna Exhibitions of 1867 and 1873. In the interval which has elapsed since the latter publication was issued Brazil has in some respects improved her position among the nations of the earth, and the compilers of the works referred to have thought it due to "the old and constant friendship which links the two countries," no less than the important commercial relations existing between them, to issue the volume under notice for the Centennial Exhibition at Philadelphia.

The work deals with almost every conceivable subject, and supplies ample information on all; but, perhaps, the chapters which will be read with most interest are those which treat of the animal, vegetable, and mineral kingdoms, the last-named being, on the whole, the most attractive. The compilers have wisely published the result of their labours in English, and, considering the great disadvantages with which they must have had to contend, we are bound to admit that the mistakes in spelling and diction are much fewer than might have been expected.

Rain and Rivers, or Hutton and Playfair against Lyell and All Comers. By Colonel George Greenwood. London: Longmans and Co.

This somewhat fantastic title introduces to us a work which may perhaps be regarded as a contribution to geological science, or perhaps as an attempt to annul the larger portion of that science, and to render its reconstruction, in anything like its present extent and importance, an impossibility. Colonel Greenwood's direct and immediate purpose is to show, in opposition to Lyell, that valleys have not been formed by the action of oceanic currents, waves, &c., prior to or during the elevation of the land, but are of atmospheric origin, having been gradually excavated by "rain and rivers." For this view scientific evidence is adduced. But the author maintains other propositions, no less zealously, though with much less foundation in facts or in sound argument. Thus he tells us that "man and the mammalia—that is, the most perfect creatures—may have existed on the land from the beginning, and *before* the first strata were formed in the sea, and consequently that Darwinism and the development-theory are myths. The land is the region of perpetual

disintegration, denudation, and destruction. The sea is the region of perpetual deposit and conservation. And sea-strata when hoisted up by heat in the form of slate, sandstone, limestone, &c., form museums of sea-life of an antiquity quite incomprehensible to man. But these museums contain sea-life only: land-animals neither live nor die in the sea; they live and die on the land. And where are the museums for the preservation of ancient land-life? There cannot be such a thing. The entire surface of the earth is perpetually vanishing, and with it the museums for the preservation of ancient land-life. The most ancient museums of land-life are caverns, filled-up lakes, bogs, and drift and alluvium—things which, geologically speaking, were formed yesterday and will be gone to-morrow. In these modern land-museums, however, the remains of man and extinct mammalia are found, and they *would* be found in more ancient land-museums if such ancient land-museums *could* exist. . . . . The whole affair is the result of the most childish confusion between space and time, between place and period. That is, because in the deposit of a certain *place* such a life only existed, we set it down that in the period when that deposit was formed that life only existed. Hence such errors as 'The age of reptiles,' 'The diluvial period,' 'The boulder period,' 'The drift period,' 'The period of invertebrates,' 'The pluvial period,' 'The gravel period,' 'The peat period,' &c." At the same time, however, the author admits that species have died out and that others have been successively created, "and at distinct times, and in comparatively modern times." Further we read—"Unless we except man (as I think we may) the existence of all organic species is not only finite, but it is transitory as compared with the existence of the globe, and it depends on second causes." Therefore the whole doctrine of the successive appearance and disappearance of species, which we have just seen denied, is after all admitted! This appearance and existence, too, is in one place spoken of as due to "second causes," and yet in another it is referred to a succession of creations!

All the knots of animal and vegetal geography are abruptly and compendiously cut. "Like the chicken-fancier who keeps his fowl-yards separate, Nature seems purposely to have contrived *different* stations with *similar* physical conditions, in order to exhibit the profuseness of her creative power in cramming all full of animal and vegetal existences with constitutions similar to those of similar but separate stations, but the species of each similar separate station differing entirely from the species of all other separate stations." We need scarcely point out how radically unscientific, or rather anti-scientific, is the spirit of this passage. If this method of accounting for the phenomena which meet us in the universe is to be accepted, why should we observe, register, classify, or seek to account for anything? Why not close our books, give our apparatus to the children for

toys, and say "things are because they are," which is merely a plainer and briefer way of expressing our author's views. We will venture to say that had he even devoted a single year to the serious study of animal geography, he would have been more chary of referring to "Nature's intentions," and would never have sought to account for the distribution of species in such a manner.

Various other utterances, more remarkable and well founded, might easily be selected, if any useful purpose could be thereby answered.

It is to be profoundly regretted that one who displays so much rashness in theorising, and who reasons in such a very peculiar manner, should not have deemed it incumbent upon him to show a little more courtesy toward those from whom he dissents. Such expressions as the "pompous Humboldt," whose "ideas appear like the ravings of madness," are happily not customary weapons in scientific controversy, and must assuredly damage the man who has the questionable taste to use them much more than the one at whose memory they are levelled.

All that is really valuable in this book might well have been compressed into much smaller compass, and certain of the sections ought never to have been written.

Hay-Fever or Summer Catarrh; its Nature and Treatment. By G. M. Beard, A.M., M.D. New York: Harper and Brothers.

We cannot presume to endorse the opinion of an old and somewhat testy friend, that "hay-fever is all humbug," although certainly we have never met with or heard of a case in private life, and know of its existence merely from medical works. Though not recognised as a distinct disease prior to 1819, it seems to have become prevalent in England, and still more in the northern part of the United States, and already attracts a considerable amount of attention. Unlike his predecessors in the enquiry as to its nature and origin, Dr. Beard holds that hay-fever is essentially a neurosis—a functional disease of the nervous system. "The debilitating influences of heat and the external irritation of vegetable and other substances (pollen from different plants, essential oils, &c.) are exciting causes merely." He does not believe in the existence of any specific suitable for all persons suffering from the disease.

It is interesting to note that in America a special Association has been formed for the investigation of this affection.

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*The Solution of the Most Important hitherto Unsolved Problems in Nature.** By J. F. Lochner. Cöln and Leipsig: E. H. Mayer.

Germany, fruitful as she is in sound scientific research, is not very rich in scientific heresies: she produces few attempts to set aside at a stroke the labours of our most illustrious philosophers, and to solve questions which they have found too difficult. She has, we believe, no "Zetetic Astronomy." no "Trinology," no "Origin of Creation." Some persons may ascribe the absence of such intellectual vagaries to a want of independent thought and a tendency to accept existing systems without challenge. We should rather seek for an explanation in the circumstance that in Germany public opinion does not encourage a man in coming forward to enlighten the world on any matter without having made it the subject of his thorough and especial study.

The work before us seems an exception. The author appears equally able to deal with unsolved problems in astronomy, physics, chemistry, biology, or geology. As a specimen of Herr Lochner's method we will take his reply to the question why the surface of our globe "consists of one-quarter land and nearly three-quarters water, while the inverse proportion would be certainly more suitable?"

To the common view that this excess of water is required to supply sufficient moisture for the support of the vegetable world, he objects that of the ascending vapours about two-thirds must fall back uselessly into the sea from which they have arisen. He further contends that "there are districts where it rains very seldom or not at all, and where everything yet flourishes. Here the vapours fall down in the night as a strong dew." But does not the author see that an abundant supply of vapour is equally needed whether the earth is to be watered by rain, dew, or by rivers? His view as to the use of the ocean is that it constitutes a kind of reserve, from which, when the human race becomes more numerous, and when even "this great America" is overpeopled, new islands and even new continents will rise up. Now, that islands have risen up and that continents have become extended appears to be beyond dispute; but unfortunately islands, and even continents, are considered, on equally good evidence, to have disappeared. What is worst of all, the lands that have been thus lost would seem to have been much more valuable and better adapted for the habitation of man than those which have been elevated in their stead. Thus Siberia is doubtless a wretched compensation for the continent that is supposed to have extended eastwards from Australia. Whether the relative proportions of land and water remain unaltered, or whether

* Die Lösung der wichtigsten bis jetzt noch unerklärten Probleme in der Natur.

the solid portion of the surface tends to increase, we are unable to decide. We must remember that the sea is no less replete with organic life than the dry land, and if we could contract it we should merely give greater scope to certain living forms at the expense of others.

In another chapter we read—"I hold therefore that the gravitative force is the finest that exists in all creation, as it consists of the ultimate atoms conceivable in space. As this matter has no intervals or pores, it can consequently contain no heat-matter. But whatever in nature contains no heat must have cold,—consequently gravitation is absolutely cold."

In short, the author—as he states in other passages—regards cold not as a mere degree of heat, low with regard to our feelings or to the ordinary temperature of objects around us, but as something actually existing in nature. This is one of the leading doctrines of Forfar's "Trinology." Nitrogen, the author considers, if compressed by a sufficient degree of cold, is changed into *infinitely small particles of iron!* "From the highest strata of the atmosphere in which this took place these particles fell, by reason of their gravity, to the lowest, and thus became diffused over the whole surface of our globe. It is therefore not to be wondered at if iron is to be found everywhere." With this sentence we will conclude our notice of this extraordinary work, leaving our readers to meditate on this new theory of the origin of iron.

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*Statistical Investigations on Mental Diseases.** By F. W. Hagen. Erlangen: Eduard Besold.

We have here an application of the statistical method to the solution of certain questions connected with the painful subject of mental alienation. After a very able Introduction, treating of medical statistics in general and of the statistics of this class of disease in particular, the author gives a short account of the asylum for central Franconia. He then discusses the influence of the duration of mental disease previous to the admission of a patient into the asylum, upon its total duration, the general mortality of the insane, the various forms of the disease, and the influence of sex, age, and civil status.† From these topics he passes to the capital point, the heredity of insanity. Here his observations lead him to a result which he rightly terms "consolatory," namely, that "the inheritance of psychic disease does not occur so frequently and so unconditionally as might be sup-

* Statistische Untersuchungen über Geistes krankheiten.

† The word which we, for want of a better term, translate by "civil status" does not refer to rank or occupation, but to the distinction between the unmarried, the married, and the widowed.

posed from the impression made by affirmative instances." The number of patients in whom a hereditary tendency could be traced was to the total number as 1 : 2·68. In the male sex alone this proportion falls to 1 : 3·05, but in the female rises to 1 : 2·34. Acute derangement is more frequently hereditary than chronic mental affections to the extent of 7 per cent. In acute, chronic, and paralytic lunacy the paternal influence is to the maternal as 100 : 103·3. Hence a tendency to mental affections in the female line is somewhat the more formidable. Parents whose madness is not hereditary have more frequently sound children than those whose disease has been inherited, and the offspring of the latter are more frequently weak-minded or idiotic. Cases where all the children die young are more frequent where the madness of the parents has not been inherited. In case of lunatic fathers the children were more liable to mental disease than the grandchildren. In case of lunatic mothers the rule is reversed. Moral degeneration, on the other hand, increases in the second generation. The only doubt capable of affecting the author's conclusions is that hereditary tendency to disease may often exist where it cannot be traced. Very few persons could show that not merely all their direct ancestors for some three or four generations back, but all the collateral branches of the family in the ascending line, were perfectly free from insanity. Indeed such an inquiry could not possibly be undertaken with success unless we were first provided with a rigid definition of insanity, and were able to say where "oddness," "eccentricity," "peculiarity"—perhaps even where genius—ends and where madness begins. There are grounds not to be altogether neglected for considering genius as a morbid product, bearing a closer semblance to insanity than is commonly dreamt of.

Another long and ably-conducted investigation is devoted to the connection between insanity and phthisis. The author finds that the insane succumb to pulmonary consumption in a fivefold higher ratio than the sane, and that, conversely, among those affected with tubercular degeneration, mental disease is five times more common than among the non-tuberculous. In a largely preponderating number of cases the mental disease precedes the pulmonary affection. Hence pulmonary consumption cannot be considered as the cause, but rather as a possible effect of insanity.

We cannot too strongly recommend this thorough-going, masterly, and truly philosophical treatise to medical men, and to all who have any interest in this important subject. We must, however, point out that the value of the book would have been greatly enhanced by a more copious table of contents, or by a good index.

Peabody Academy of Science. Fourth Memoir. *Fresh-water Shell-Mounds of the St. John's River, Florida.* By JEFFRIES WYMAN. Salem, Mass.: published by the Peabody Academy of Science.

ZEALOUS and praiseworthy attempts are being made in America to ascertain the history of the tribes who once occupied the Western Continent, but who have faded away, either since the arrival of European settlers or in far earlier periods. Much important evidence it is to be feared has passed hopelessly away. The ignorant bigotry of the earlier Spanish settlers destroyed the most important monuments and inscriptions, and the sloth of their successors has allowed much more to perish. But much may be done by a diligent use of what remains, and we may yet learn no little of the nature and habits of nations who inhabited North America thousands of years ago, and who at first sight appear to have died and left no sign.

"Shell-mounds" are very widely distributed. In a certain stage of culture—or rather of the absence of culture—mankind seem to have established themselves on the shores of the sea and of certain lakes, and to have subsisted mainly upon "shell-fish." This was undoubtedly before the discovery of suitable tools had rendered agriculture possible—probably before weapons for the chase were invented. The peninsula of Florida contains numerous of this class, both on the sea-coast and along the banks of rivers, composed of a mixture of shells and bones of animals which have been used as food. These mounds contain fireplaces, and various tools made of stone, bone, or shell. The stone implements are rarely met with in the mounds themselves, but are more abundant on the surface, and are of a very rude character. The bone and shell tools are of superior workmanship, but are believed not to have been made by the tribes who formed these heaps, but to have been introduced by immigrants from the north. In the more recent mounds, though not in the oldest, fragments of pottery of a very rude kind have been discovered. Evidences of the prevalence of cannibalism are unmistakable, as bones of human beings are found, broken up in the same manner as those of other animals. Bones and teeth of extinct animals are found,—such as the mastodon, elephant, horse, and ox, but these have undergone changes which show that they were not contemporary with the builders of the mounds. It may be approximately ascertained that some of these mounds were finished at least two or three centuries before the arrival of the first European settlers. The balance of evidence inclines to show that the formers of these mounds were not the people found in Florida by the Spaniards and the French, but a more primitive race, having apparently no knowledge of agriculture.

Elementary Arithmetic, with Brief Notices of its History. By R. Potts, M.A. Cambridge: W. Metcalfe and Son. London: the National Society's Depository.

This work differs strikingly in some respects from the ordinary arithmetical treatises used in schools. The first of the twelve sections of which the work consists is devoted to an introductory dissertation on number, on numerals, and the history of arithmetic, and contains much curious and interesting information. The second section treats on money, and gives a history of the British coinage from Tasciovanus, king of the Trinobantes (B.C. 30 to A.D. 5), down to the present day. Section III. is devoted to the consideration of weights and measures. The writer is evidently hostile to the metric system, and we may venture to say that one of the main objects of the work before us is to delay or altogether prevent its introduction into England. We regret that such a spirit should find scope in a work which, if we mistake not, is adopted as a text-book in the "National" schools of England. What advantages are to follow from the retention of our present involved and cumbrous system the author does not show, and we cannot help suspecting that, both on his part and on that of the three eminent men of science who share his opinions, prejudice—perhaps scarcely conscious—is in reality the moving power. With many very well meaning but not over clear-headed people we know that the metric system is in some mysterious way associated with atheism, Robespierre, and the reign of terror. The proposal to abolish the so-called "compound" rules of arithmetic, and to make all addition simple addition, is supposed to involve a covert plot against the altar and the throne. Section IV. treats of time and its divisions, Section V. of logarithms, and it is not until Section VI. that the consideration of the ordinary rules of arithmetic is taken up.

Were it not for the attack upon the metric system we should feel bound to give the work our cordial recommendation, and as it is we must bear witness to the clearness with which the author unfolds every successive portion of his subject, and aims very successfully at rendering each step thoroughly intelligible to the student.

Monthly Notices of the Papers and Proceedings of the Royal Society of Tasmania, for 1874. Tasmania: Mercury Office, Hobart Town.

The amount of valuable work which may be done by our colonial scientific societies is immense. To them the learned world must look for detailed and accurate accounts of the fauna and

flora, both fossil and recent, of interesting regions concerning which our knowledge is a mere sketch, even if accurate. We observe that the Society is engaged with the formation of a museum, which we hope will become rich in local collections in all departments. This is the more important as many plants and animals in the colonies may soon become extirpated, owing, on the one hand, to the wanton destruction perpetrated by ignorant men, and on the other to the importation of aggressive plants and destructive animals. The Society pays also considerable attention to the acclimatisation of products which may be of commercial value.

Among the papers communicated we may point out as particularly interesting one by the Rev. J. E. Tenison Woods, F.L.S. and F.G.S., on the "Physical and Zoological Relations between Australia and Tasmania." The author compares especially the eastern portion of the Australian continent with the island. He considers that Tasmania unites, in her natural history, features which are characteristic of distinct provinces in Australia. Eastern Australia he divides into three zones;—The coast region, characterised by a genial humid climate, with a vegetation in the temperate regions of almost tropical luxuriance, and generally Asiatic in its features, becoming more decidedly so as we advance northwards. The second zone is the table-land with the mountains, having a distinct vegetation, alpine in the southern portions, and in the northern more essentially Australian and less Asiatic than the coast regions in the same latitude. The innermost or desert zone extends to the south coast, except where mountains intervene. It is more characteristically and peculiarly Australian in its features than the other zones, and possesses fewer forms common to other countries. The two first-mentioned regions are well represented in Tasmania, with modifications which tend to show that its separation from the mainland is not of very recent origin. The climate is much affected by the gap of Bass Straits, and the vegetation is much less luxuriant and less tropical in character. Its affinities seem to point rather to Polynesia or New Zealand than to Asia. The large and beautiful diurnal Lepidoptera common in Eastern Australia are either rare or totally unknown in Tasmania. The land-shells of Tasmania are all strongly-marked species, with very little affinity with those of the East coast, but having analogues in New Zealand.

This volume also contains a catalogue of the plants of Tasmania, drawn up by Baron Mueller. About fifty species have been discovered since the completion of Dr. Hooker's great work in 1860.

Abstract of Results of a Study of the Genera Geomys *and* Thomomys, *with Addenda on the Osteology of the Geomyidæ and on the Habits of* Geomys Tuza. By Dr. Elliott Coues, U.S. Army. Washington: Government Printing-Office.

A valuable monograph of two nearly allied genera of rodents. It is to be regretted that the author, in his paper on the habits of *Geomys Tuza*, has retained for it the vulgar and misleading name of "salamander," by which it appears to be known in Florida and Georgia. The animal is very injurious, undermining the heaps of potatoes buried in the fields, and carrying away the store.

United States Geological Survey of the Territories. A Monograph of the Geometrid Moths or Phalænidæ of the United States. By A. S. Packard, Jun., M.D. Washington: Government Printing-Office.

We always feel the most sincere respect for the man who has the courage and the wisdom to devote himself to the investigation of some one scientific question, however apparently limited, with the determination to ascertain all that can be known on the subject. A monograph may, indeed, be a less ready way to reputation than a popular manual, or a brilliant but crude and superficial generalisation. But it is a far more valuable contribution to human knowledge.

There may be persons so blinded by considerations of immediate utility, and so ignorant of the forces at work in this world of ours, as to sneer at the study of insects, and to feel aghast at a goodly quarto devoted to an exposition of the structure, classification, and geographical distribution of one particular class of moths in one section of the western hemisphere. We must, however, remind such persons that of all our enemies in the animal kingdom insects are the most formidable,—capable, indeed, of making life unendurable, of destroying the crops of whole provinces, and of exposing nations to the horrors of famine. To contend with such opponents of our well-being we must possess a full and accurate knowledge of their structure, constitution, and habits. Thus, even on the lowest utilitarian ground, Entomology is one of the most important of studies. This truth has been fully recognised in the United States, where fearful ravages have been committed by various insect-pests, and where, in consequence, both the Federal Government and the State legislatures have been aroused to the necessity of action.

The reader, if not an entomologist, may ask what is a geometrid moth? If he will observe caterpillars in the fields and

woods he will find that some of them have, to speak popularly, an almost uninterrupted series of feet, sixteen in number, from head to tail, and when creeping always keep their bodies in close contact with the leaf or branch upon which they are supported. Others, of which the well-known gooseberry caterpillar (*Abraxas grossulariata*) have merely ten feet, arranged in two groups, one at the anterior and the other at the posterior extremity. When crawling they grasp the leaf or other object firmly with their fore feet, and then bring the hind feet close up to the former, so as to bend the body into a kind of loop, and thus advance by a kind of striding or spanning movement. The moths developed from such caterpillars are called Geometridæ: they are a very numerous family, though, not being remarkable either for size or from brilliant coloration, they are often overlooked. In Europe eight hundred species have been enumerated. In the work before us between three and four hundred species are described, but the author is so far from thinking his subject exhausted that he considers a thousand species may be found in the North-American continent. His region agrees with Mr. Sclater's Nearctic province,—in other words, all North America beyond the tropic of Cancer.

Commencing with a history of the family as characterised and subdivided by entomological authorities, he passes on to the comparative anatomy of the various groups, and thence to the description of species. The geographical distribution of the Geometridæ of the United States is finally considered. Unlike Mr. Sclater and Mr. Wallace, he subdivides the region not into four, but two provinces, the Arctic and the North Temperate Realm. Tables are given of the species peculiar to Greenland and Labrador; of the subarctic species common to Eastern and Western America, and to Europe and Asia; of the species inhabiting the eastern province of the north-temperate realm; of those found in the limits of the Alleghanian and Carolinian faunæ; of those occurring south of the isothermal of 60°, the territory corresponding to Le Conte's Southern province; and those of the Western province.

The author calls attention to two features of interest in the distribution of the insects of the Pacific slope, viz., the absence of forms characteristic of China and Japan, and the presence of some European types which do not occur in the Atlantic province,—features which may be traced in other Lepidopterous families, in other orders of insects. In order to illustrate the relations between the Geometridæ of different regions, he gives tables of the species (18 in number) common to temperate America and Europe, of the species common to the Eastern and Western provinces of North America, of those peculiar to North and South America, of those occurring in Central and South America—the Neotropical region, and of European genera not as yet discovered in the United States.

The following is an interesting contribution to our as yet very scanty knowledge of what may be called the local physiognomy of the faunæ of different countries :—" In all the species enumerated in the following list—25 in number—the Colorado examples (when the species have been found to occur there) and the Pacific Coast individuals are larger, and in some cases with longer and more pointed wings, than in those from Labrador and the Eastern Atlantic States, and in a few instances show a tendency to become lighter in colour."

The following passage is also worthy of special attention :—" It will be seen from the facts we have presented that the moths probably follow, as regards size, a law the reverse of that established by Prof. Baird for the birds and mammals, who shows that they decrease in size southwards, though his law of increase in the length of certain peripheral parts westward also obtains in the Lepidoptera. The increase in size westwards is of course equivalent to the well-known southward increase of size in insects, though in a few species of insects the Coloradan and Californian specimens are larger than Floridian and Texan insects of the same species. Of the insects mentioned in the list *Plusia Hockenwarthi* is the clearest example (1) of the law of increase in size westwards and southwards ; (2) increase in length of peripheral parts ; (3) brighter, deeper colours westward."

Dr. Packard is decidedly opposed to the theory of Heer, of an " inter-continental bridge between the temperate zones of America and Europe, and of America and Asia."

The work is admirably and abundantly illustrated : twelve large plates, with many hundred figures, represent not merely the moths as described, but the characteristic venation of their wings, and other important morphological details. Upon Dr. Packard the work reflects the highest credit. He has, evidently with a most lavish expenditure of time and labour, produced a most valuable contribution to zoological science, in which accuracy of detail and a large and philosophic spirit are equally conspicuous. Nor, whilst expressing our appreciation of the author, must we forget to commemorate the far-sighted and enlightened policy of the American Government in promoting such researches, and the more than royal munificence with which this and similar volumes are presented to learned societies, public libraries, and the conductors of scientific journals throughout the civilised world. Were this example followed by governments which have even greater opportunities, an absolutely priceless selection of materials would be placed within the reach of scientific men, and the verification of theories would be rendered a comparatively easy task.

The Sun — Ruler, Force, Light, and Life of the Planetary System. By Richard A. Proctor, Author of "Other Worlds than Ours," "Saturn and its System," "Light Science for Leisure Hours," &c. With nine Lithographic Plates (seven coloured), and one hundred Drawings on Wood. Third Edition. Longmans and Co. 1876.

This, as the Preface informs us, is a carefully revised new edition of Mr. Proctor's well-known treatise, with some important sections added, and from which the Appendix concerning the transit of Venus has been removed, as the purpose of that Essay has been accomplished.

We are glad that Mr. Proctor has thus re-issued his work on the Sun, and has not written a new book on the same subject with another title. In common with the majority of the most sincere admirers of Mr. Proctor's splendid abilities, we have watched his literary career with something akin to fear and trembling. So many new books following so closely one upon the other, with imminent risk of treading upon the heels of their predecessors, and mutually tripping each other, may ultimately overtask and dilute the energies even of Mr. Proctor's powerful intellect. It is therefore very satisfactory to find that he is working upon standard books as well as bookseller's books, or books for the season. This work with its companion treatises, "The Moon" and "Saturn and its System," will doubtless hold their ground as standard works on their respective subjects, and be followed, we hope, with corresponding standard treatises on the other members of the solar system.

Such works unfortunately are, at the outset, far less remunerative to the author than Mudie-books, that run for a season or two, and must be replaced by something that seems to be different, and must be, above all things, *new*, if only in title; but there can be no doubt that carefully-written and slow-selling standard works are ultimately, by virtue of their permanency, the most profitable, not only as regards reputation, but even as a source of income to the author, who, unlike the publisher, cannot take up another writer, and trade upon another reputation when his own is worked out.

The great merit of this work, as of the carefully-written portion of Mr. Proctor's other productions, is that it is popular without any sacrifice of scientific precision or soundness; and this popularity is attained without treating his readers as though they were babies, with any overwrought affectation of extreme simplicity. The facts and phenomena are plainly and directly described, without any excess of those "familiar illustrations" with which so many of our modern popular treatises are so thickly padded, to the confusion of the student and the exclusion of important information on the actual subject of the work.

Mr. Proctor's mathematical attainments have not deprived him of the use of unsophisticated language and argument; he can reason as well as formulate; he does not use mathematical expressions as a *substitute* for descriptive reasoning, but only when they are actually demanded by the essentially mathematical nature of his subject. Even then he uses the simplest forms, such as most ordinary readers can follow with a slight effort of attention.

The first chapter of this book, dealing with the strictly mathematical subject of the sun's distance and dimensions, is a notable example of this admirable characteristic of Mr. Proctor's dissertations; and similar powers of elucidation are displayed in the third chapter, on analysing sunlight, We know not where else the unlearned reader could find an equally profound yet clear and simple treatment of these subjects.

The interest of the later chapters is considerably enhanced by the author's free handling of the theoretical elements of the subjects. Scientific writers of the patronising school are too apt to assume that debatable hypotheses are above the reach of the populace whom they condescend to instruct. Mr. Proctor evidently proceeds on almost an opposite principle, and treats his readers with quite a banquet of speculative viands, leaving them to choose those that are the most agreeable to their own intellectual palates. This adds greatly to the interest of his books. Many who would fall asleep over a detail of bare physical facts and quantities are kept awake by the discussion of contending hypotheses, and facts which would otherwise slip from their memory are fixed there by their necessary association with an exciting controversy.

The absence of a full index is a sad defect in this book. The table of contents is good enough, fuller than usual, but this can never supersede the necessity for an alphabetical index to every standard work that is to be used for the purpose of obtaining solid information.

It may be that Mr. Proctor is a bad index maker, or too busy to attend to this mechanical department of authorship. If so, he would do well to call in the help of a friend who has the faculty of index-making. It is by no means necessary that the author should make his own index. The best index extant is that which was made by the late Robert Cox, of Edinburgh, for the "Encyclopædia Britannica:" and the next best with which we are acquainted is that appended to his own work on "Sabbath Laws and Sabbath Duties," a pamphlet of 16 pages with 547 pages of Appendix; the pamphlet in *pica*, the Appendix in *small pica*, and the voluminous notes to the Appendix in very close *brevier*.

Lessons in Electricity at the Royal Institution, 1875-76. By John Tyndall, D.C.L., LL.D., F.R.S. London: Longmans and Co. 1876.

This little volume contains, with slight modifications, the substance of Prof. Tyndall's Christmas Lectures given to a juvenile auditory. We have been accustomed to go to the Royal Institution in order not only to hear the newest and most original matter in the world of science, but to see the most finished and elaborate apparatus; to see the simplest things projected on the screen by means of the electric light, and to see the least wants and wishes of the lecturer anticipated by a host of ready assistants. But in the present instance it has been the aim of the lecturer to show how much can be done by means of the simplest and most inexpensive apparatus—a priced list of which is given at the end of the volume, the total amounting to £5 10s.

"I had heard doubts expressed," says Dr. Tyndall in the Preface, "as to the value of science-teaching in schools, and I had heard objections urged on the score of the expensiveness of apparatus. Both doubts and objections would, I considered, be most practically met by showing what could be done in the way of discipline and instruction, by experimental lessons involving the use of apparatus so simple and inexpensive as to be within everybody's reach." The Lectures are entirely confined to frictional electricity, and of this the book gives a very good elementary account, illustrated by many original and most ingenious experiments.

The contrivances are always of the simplest, and can be made by any ordinarily ingenious person: sticks of glass, sealing-wax, and gutta-percha, a lath balanced on an egg; a simple electroscope with Dutch metal leaves; eggs for conical conductors, and apples for spherical conductors: these are some of the appliances of which the author makes use. We notice on p. 28 a very ingenious experiment: a funnel with a stem of small bore is filled with fine silver sand, which runs out in a continuous stream; but if a wire proceeding from a rubbed glass tube be connected with the sand in the funnel, the particles of sand fly out from one another as they descend by self-repulsion. We strongly and cordially recommend this capital little treatise to all boys who possess a scientific turn of mind, and they cannot do better than make for themselves the simple apparatus necessary for the performance of the principal experiments in the book, and then work through the book during the long winter days of the Christmas holidays.

Elements of Physical Manipulation. By EDWARD C. PICKERING, Thayer Professor of Physics in the Massachusetts Institute of Technology. Part II. Macmillan. 1876.

THE first portion of this work was published three years ago, and comprised the Mechanics of Solids, Liquids, and Gases, Sound, and Light. The present volume completes the cycle of such work as may be performed in a physical laboratory, and includes Electricity, Heat, Mechanical Engineering, Meteorology, Practical Astronomy, Lantern Projections, and an Appendix containing various Tables.

A fourth of the work is given to Electricity. Commencing with a short description of batteries, keys, plugs, connections, switches, and commutators, he passes to the method of using them, and thence to the telegraph. The Morse telegraph only is described, and the account is somewhat meagre, but the testing of telegraphs and of submarine cables is discussed further on. Altogether, that part of the work devoted to electrical manipulation is somewhat disappointing, and it is quite insufficient for the present wants of students.

The section on Heat commences with an account of testing thermometers, and of the determination of the expansion of solids, liquids, and gases. The experiment (No. 127, p. 82) for determining the change of volume by fusion, as there described, is quite impracticable and impossible, and no student could obtain a satisfactory result by the use of it. Neither does the small accompanying woodcut make the matter much clearer. Under the head of pyrometers we find (in a space of two pages) an account of Wedgwood's pyrometer, the use of which has been long abandoned, also of the thermopile pyrometer, the pyrometer depending on the specific heat of platinum, and Siemens's pyrometer; but the accounts are so slight as to be useless without a considerable amount of collateral information obtained from some other source. The appliance recently described for illustrating the mechanical equivalent of heat as a lecture experiment is figured and described, but the name of the originator of the apparatus is not once mentioned.

The section on mechanical engineering is a novel feature in a work on physical manipulation. The author accounts for it as follows:—"Physical problems which are to be solved on a large scale require a thorough knowledge of mechanical engineering. To this class belong also those which have the greatest pecuniary value. The methods of conducting such experiments also are often so faulty that a brief description of how they should be performed will not seem out of place." The proper care of a boiler and of an engine is first discussed. The author points out that the loss of heat by condensation and radiation when steam passes through a great length of piping is much more considerable than is often supposed. By covering the pipes with canvass, or felt,

or gypsum, the loss of heat may, to a great extent, be prevented, but the pipe of course takes a longer time to heat. A convenient form of friction break for directly determining the amount of work done by an engine is described on p. 126, and this is followed by an account of means of determining the speed of the piston-rod, fly-wheel, and shafting, and the friction of belts and pulleys. The space given to the strength of materials is small, but this branch of the subject is always treated of at length in books on applied mechanics.

Under the head of "Meteorology" an account is given not only of the determination of the more ordinary factors, but also of the three magnetic elements, and of the electricity of the air. For this latter determination Prof. Pickering proposes two instruments, one of which assumes the same electrical potential as the air, while the second measures this potential. This is usually effected by a Thomson's quadrant electrometer, or a Peltier if less accuracy is required. To collect the electricity an insulated vessel from which water slowly drops may be used, or a burning match made of blotting-paper dipped in nitrate of lead. During the progress of a distant thunder-storm the electrometer shows a sudden change of potential after each flash of lightning.

In the section on "Practical Astronomy" the author discusses the method of using the sextant for determining latitude, longitude, and time. Also time by the transit instrument, and latitude. A long account is given of the mode of using the equatorial telescope, and, finally, of the method of spectrum analysis applied to the stars.

A short section on "Lantern Projections" describes the various means of throwing photographs or images of apparatus upon a screen by means of an intense source of light, and a convex lens. As an apology for introducing the subject, the author remarks, "During the last ten years a new era has arisen in the illustration of lectures by the general introduction of the magic lantern as a means of demonstration. Not only in science, but in the mechanical arts, in architecture, and, in fact, in any subject susceptible of illustration by engravings or photographs, a few glass plates which may be carried in the hand will interest and instruct an audience more than the finest diagrams, which are, moreover, far more cumbrous and expensive. It is, therefore, desirable that every one who may have occasion to address an audience should be able to manage a lantern, and to project photographs on the screen. Again, especially in physical experiments, many objects are so minute that they cannot well be shown to a large number of persons, and an enlarged image of these may often be thrown on the screen, and thus be seen by hundreds at a time." The best methods of using sunlight, the electric light, the magnesium light, and the oxyhydrogen, or calcium light (as the author calls it) are described. For a

lantern a wooden box about one foot in the side is recommended, furnished with various apertures capable of being closed by small sliding doors. Under the most favourable circumstances only about 12 per cent of the light emitted by the source can be made to fall on the screen. The author recommends the use of the screen as a blackboard; and he gives a rather interesting account of objects for projection. Designs may be rapidly drawn on blackened glass, or on films of gelatine. Models may also be well shown by projection if care be taken to make them lie in one plane. The author remarks that it is an interesting experiment to project the light itself upon the screen; and if this be the sun, sun spots may be easily shown by the means. He further observes that by this means "during a partial eclipse the phenomena may be watched by a large number at a time." But, surely, in the latter case it would be absurd to watch an eclipse in this manner in a room, when it is possible to see the eclipse and the accompanying phenomena in the open air. Again, chromatropes, kaleidoscopes, and the growth of crystals are common objects for the lantern. The vertical lantern is a useful appliance for projecting on a screen such things as magnetic curves. A lantern galvanometer is also described.

The final fifty pages of the book are devoted to Appendices; the first of which relates to Electricity, while the second gives a most useful set of tables, of Squares, Cubes, Reciprocals, Powers, Logarithms, Natural Sines, Natural Tangents, Logarithmic Sines, Logarithmic Tangents, Constants, Properties of the Metals, Properties of Liquids and Gases, Hydrometer Tables, Temperatures, Pressure of Vapours, Wet and Dry Bulb, the Solar System, Double Stars, Clusters and Nebulæ (with their Right Ascension and Declination). The constants are nearly all given in accordance with the metric system. Among the temperatures we find a cherry-red heat given as 900° C. (1650° F.), a yellow heat as 1200° C. (2200° F.), and a white heat as 1300° C. (2400° F.). It is much to be wished that new and accurate determinations of high temperatures were made by Siemens's electrical pyrometer.

The final Appendix contains some very useful hints regarding the cost and fitting up of a physical laboratory, which clearly prove that the cost of a physical laboratory ought not to much exceed that of a chemical laboratory, while the keeping up of it is certainly less, as there is a less consumption of material. A few pages (p. 296 *et seq.*) are well filled with a list of the most standard works of reference of each of the physical sciences; also of tables, catalogues, and periodicals. Finally, we have a list of a hundred additional experiments, of which the following are examples:—

"214. Measure the velocity of the bullet from a revolver, parlour rifle, crossbow, or catapult, with a ballistic pendulum.

If the catapult is used at all, attach it to the table, and determine the effect of drawing the spring by known amounts, and also by varying the weight of the ball."

"241. Devise a form of photometer for measuring the amount of light reflected at various angles by polished surfaces, in which a single light only shall be used."

"292. Measure the density of fog, or the amount of light absorbed by layers of various thicknesses."

At a time like the present, when physical laboratories are making their appearance at our universities, and even at some of our public schools, Prof. Pickering's book is a useful contribution to Science. It seems to us, however, to be far too meagre, and its value would be greatly enhanced by the introduction of really good woodcuts of the nature of those to be found in Deschanel. There can be no doubt that the volume will grow with each succeeding edition, and many improvements may be then introduced; meanwhile we recommend it to students who work in a physical laboratory as a really useful book, where they can apply for information concerning any part that is obscure to the professor in charge of the laboratory.

PROGRESS IN SCIENCE.

PHYSICS.

Light.—Some useful experiments showing the action of Light on Ebonite have been described in "Nature," by Prof. McLeod, of the Royal Indian Engineering College, Cooper's Hill. It is, he says, well known to electricians that the insulating power of ebonite gradually diminishes, in consequence of the formation of a conducting layer of sulphuric acid on the surface; but it is not so well known that exposure to light facilitates this change, if indeed it is not an essential condition. He noticed that an ebonite plate electric machine which had been kept in a light room had changed in colour, except on those portions which had been protected from light by the rubbers. The exposed surface acquired a brown colour, and the machine acted very badly. On cleaning the plate with a hot solution of caustic soda, large quantities of ammonia were evolved, and the brown surface became softened, so that it could be easily scraped off. He therefore cut a plate of ebonite into four pieces, each about 52 m.m. long, 22 m.m. wide, and 8·5 m.m. thick, and one-half of each piece was varnished with an alcoholic solution of shellac. Two pieces were placed in test-tubes plugged with cotton-wool, the other two being sealed hermetically in similar tubes. One of each of these tubes were placed in a dark drawer, and the other pair exposed to light in the laboratory, and during the latter part of the experiment to direct sunlight. About nine months afterwards the tubes were opened, the ebonite washed with water, and the amount of acid determined by standard solution of caustic soda. No trace of acid could be detected on either of the pieces of ebonite which had been kept in the dark; on the one which had been exposed to light in the closed tube 0·343 m.grm. of sulphuric acid was found; and on the other, which for three weeks had been exposed to both light and air, 2·646 m.grms.

We learn, from the same journal, that Dr. Janssen is devising the construction of an automatic photographic revolver which will take a photograph of the sun every hour, each day of the year, from sunrise to sunset. The photographs which will be taken under cloudy conditions will be useless so far as sun-spots are concerned, but they might be utilised for meteorological purposes. The others will be kept and tabulated. The advantage of this plan is that it will dispense with any observer, and will obtain a mechanical regularity.

We have received from Mr. Todd, the Postmaster-General and Superintendent of Telegraphs and Government Astronomer of South Australia, a pamphlet entitled "South Australia, its Observatory and Meteorology." The Observatory is situated on the West Park lands, having the city of Adelaide and the Mount Lofty Ranges on the east, and St. Vincent's Gulf—towards which the land gently slopes—at a distance of about 5 miles on the west. Until recently its operations were chiefly confined to Meteorology, but advantage was taken of the transit of Venus in 1874 to procure a 10-feet equatorial by Cooke and Son, of York, and other instruments will shortly be added. Mr. Todd is anxious to turn the Observatory to account in the promotion of high-class education, by the delivery at the Observatory of lectures to students on Physical Science. In a recent Report to the Government he says that the Observatory is required not so much for the furtherance of astronomical science as for educational purposes. It will be able to render valuable aid in those fields of astronomical research which do not involve continuous observation or heavy computations—such work, for instance, as solar and stellar spectroscopy, sun-spots, double stars, and what Sir G. B. Airy has pointed out as a great want, observations of occultations, eclipses, and transits of Jupiter's

satellites; but beyond this Mr. Todd is anxious to see the Observatory popularised as a School of Physical Science, at which regular courses of lectures should be delivered on practical and physical astronomy, navigation, meteorology, magnetism, electricity, heat, light, and optics.

Microscopy.—Prof. H. L. Smith, of Hobart College, Geneva, N.Y., makes use of the sheet wax of which artificial flowers are formed for mounting opaque objects, such as the Foraminiferæ, &c. Disks of suitable colour are punched out and pressed on a glass slide, which is warmed until the wax melts: when cool it will be found firmly attached to the slide, and the surface quite smooth and dead. A curtain-ring is pressed into the wax while warm, to form the cell-wall, and the whole finished off with Brunswick black outside in the usual manner. To attach the objects, a minute drop of turpentine is applied to the wax, and before it is quite dry the object is placed on the softened wax, and when thoroughly dry it will be found so strongly attached that a violent blow or fall will not dislodge it. While this is being done the Brunswick black on the ring will have set sufficiently to fasten the cover, which should be of such a size as to rest, not on the top of the ring, but to slip just within, so that its surface will be flush with the top of the ring. When the cover is pressed home the whole may at once, without any danger of running in, be finished with the black varnish.

A new process of histological staining has been communicated to the Quekett Microscopical Club, by Frances Elizabeth Hoggan, M.D. The tissues to be stained are membranes or soft sections, which may be either fresh, frozen, hardened in alcohol, or hardened by the picric acid and gum process; but such hardening agents as the chloride of gold, or any chromate whatsoever, are inadmissible. The colouring agents required are—a 1 or 2 per cent solution of perchloride of iron in distilled water or alcohol ("*tincture of steel*"), and a solution of similar strength of pyrogallic acid in water or alcohol, the latter fluid being preferable in both cases. The section or membrane to be stained is first to be treated for one or two minutes with alcohol; the iron solution is filtered upon it, allowed to remain for a couple of minutes, and then poured off. The pyrogallic acid solution is then filtered in a similar way upon it, and in the course of a minute or two—the desired depth of staining having been obtained—the tissue is washed, and may be mounted in the usual manner, in glycerin, balsam, or varnish. The nuclei and nucleoli will be found coloured black, and the cell substance will also be coloured more or less, according to the age and other conditions of the cell. A bluish tint may be given to them by washing the section with an alkaline water. The whole process is speedy, simple, and permanent. The staining may be accomplished in five minutes, and the materials can be bought at any country druggist's shop.

Dr. J. J. Woodward, of the Medical Department, United States Army, has made use of photography for micrometry where extreme delicacy is needed, as in the measurement of blood corpuscles for medico-legal cases. A glass plate is ruled in hundredths, thousandths, and five-thousandths of an inch: upon this a thin film of the blood to be examined is spread; this is best done by means of the edge of a glass slide. If dried blood-stains are to be examined, as in criminal cases, the fragments are soaked out by the usual processes. The photograph is taken, and the ruled lines appear with the image of the blood-corpuscles: the ordinary stage micrometer, covered with thin glass, cannot be employed, as the lines would not be in focus with the object. The measurements are made on the negative, under a magnifying glass, by means of a transparent scale ruled in hundredths of an inch on a thin strip of horn. To avoid parallax, the ruled side of the scale is placed in contact with the varnished film of the negative. The process is far superior in accuracy to the results obtained by any form of eye-piece micrometer, and to those possessing the necessary photographic appliances occupies little or no more time.

In the "American Journal of Microscopy" the Editor, Prof. Phin, has made some severe strictures upon the statements made by Prof. P. B. Wilson, of Washington University, Baltimore, Md., concerning the value of infusorial

earth as an ingredient of fertilisers for cereal crops. In the October No. of this journal was an article, by Mr. W. H. Wahl, entitled "Infusorial Earth and its Industrial Uses," in the course of which incidental allusion was made to Prof. Wilson's alleged discovery: this was in substance that, having examined the straw from wheat grown upon land fertilised with a manure of which infusorial earth was an ingredient, he found, upon placing under the microscope what remained of the straw after it had been treated with nitric acid, that it consisted wholly of the siliceous shields of Diatomaceæ. Prof. Wilson alleged, in other words, that these diatoms, when incorporated with the soil, are taken up by the plant *as such*, without undergoing previous solution, and to prove his assertion he published an engraving illustrating quite a number of organic forms that he had detected. Upon the strength of these alleged observations Prof. Wilson advanced certain conclusions greatly at variance with current scientific opinion, which were stated in the article in question. While not sharing the opinions as to the significance of the presence of these organic forms in (Kunkel) straw, and expressing doubts upon the accuracy of his inferences, the writer did not presume to entertain a doubt as to the *genuineness* of Prof. Wilson's asserted observations. "The American Journal of Science and Arts" also admitted into its pages an elaborate article by Prof. Wilson, bearing the title "Silica of Grasses and other Plants carried up as Diatoms or other Siliceous Grains, and not in Solution or as Soluble Silicates." Prof. Phin emphatically denies the genuineness of Prof. Wilson's alleged observations. The following extracts from Prof. Phin's article are sufficiently explicit upon this point:—A single glance at the engraving to which he so confidently refers is sufficient to convince any microscopist that Prof. P. B. Wilson never saw 'upon the field of his microscope,' under the circumstances which he has described, the objects which he has delineated. This is a bold assertion and a severe accusation, but the proof is simple and unimpeachable." Prof. Phin then proceeds to establish his accusation by showing that one of the forms (*Bacillaria*) figured on the plate is figured as it exists only in the living condition (the organic matter, it will be remembered, has been destroyed by treatment of the straw with nitric acid), the frustules being joined together in the peculiar way which has given to this form the specific name *paradoxa*. "For this diatom," says Prof. Phin, "to have passed through a bath of nitric acid, and come out in the condition figured, would have been almost as great a miracle as passing Shadrach, Meshach, and Abednego unscathed through the fiery furnace of Nebuchadnezzar." He (Prof. Phin) affirms also that a *calcareous* Foraminifer is figured among the forms on Prof. Wilson's plate. If this be the case, we should require another miracle to have saved from destruction in the nitric acid this particular object; for it is a fact of common knowledge that carbonate of lime is energetically dissolved by nitric acid, even in the cold. Finally, Prof. Phin appends to his own criticism the statement of "one of our ablest diatomists," who declares that the thirty-six forms which Prof. Wilson has "carefully sketched" are composed of a *mixture* of marine and fresh-water diatoms, sponge spicules, a little of the siliceous cuticle of straw, and a Foraminifer. "*Only one form*," he remarks, "*belongs to the Virginia deposit with which Kunkel's field was fertilised—and which is exclusively marine.*" We hope that Professor Wilson will put us in a position to give his explanation in our next issue.

ENGINEERING.

In a pamphlet entitled "The River Clyde, an Historical Description of the Rise and Progress of the Harbour of Glasgow, and of the Improvement of the River from Glasgow to Port Glasgow," Mr. James Deas, M. Inst. C.E., gives an account of the successive improvements of the River Clyde, which in its present state may be deemed "as much an artificial navigation as the Suez Canal." Few, indeed, of the multitudes of strangers who annually visit Glasgow, and see the forest of masts in its harbour, would imagine that "so recently as one hundred years ago the river was almost in a state of Nature (!) and was fordable on foot at Dumbuck Ford, more than 12 miles below Glasgow." Unfortunately, whilst the Clyde has been widened and deepened, it

has been polluted *pari passû*, and most certainly cannot now be accused of being in "a state of Nature." The author admits "The rights of salmon-fishing in the river were carefully protected in the earlier Acts of Parliament, and fishing-stations were numerous and of much value,—one station, with its hut, being within the precincts of the harbour,—but the whirr of the paddle, the churn of the screw, and, above all, the sewage from the vast population in the area which the river drains, together with the deleterious liquid refuse from numerous manufactories of all kinds, have driven away that much-prized fish." It is very true that the preservation of fish is not the highest national concern. We may import, or in case of need even dispense with salmon, but unfortunately we cannot forego our manufactories. Still there is no occasion that the pollution of rivers shall be accepted as a necessity. It may not be possible to restore the Clyde to its ancient crystalline purity, but its present condition is capable of vast improvement. It is very possible that the outlay for dredging the bed of the river might be notably reduced were the suspended impurities not washed down by the sewers duly arrested. The improvements in the navigation of the Clyde have not been effected without a very serious outlay. The total expenditure of the Trustees since 1770 amounts to £5,594,981, of which £1,390,947, or nearly one-fourth, has been spent in interest on loans. The present debt of the Trust (June 30th, 1872) amounts to £2,151,557. The expenditure for dredging alone has been upwards of £500,000, and during the last twenty-eight years 14,609,454 cubic yards of material, or upwards of 18,000,000 tons, have been dredged and removed. One result of the improvements carried out on the river since 1758 has been the gradual lowering of the level of low water in Glasgow Harbour to the amount of 8 feet, and even since 1853 it has fallen at least 13 inches. The author ascribes this change to the great extent to which deepening, widening, and straightening have been recently carried on in the lower reaches of the river, thus enabling the ebb tides to hurry out to sea more rapidly than heretofore. On the other hand, the high-water level of spring tides is now about 10 inches higher than in 1853. One evil which has been completely overcome is the liability of certain low-lying portions of the city of Glasgow to inundations. Since 1856 the river has not been over the harbour-quays.

The blowing-up of the reef at Hallett's Point, which formed such a dangerous obstruction in the Hell Gate Channel, and greatly hindered navigation through Long Island Sound, has been truly termed one of the most successful engineering exploits of the century. We are indebted to the "Popular Science Monthly" for the following concise account of the experiment. The reef was of an irregular crescent shape, some 700 feet long, and extending out 300 feet into the channel, with an area of about three acres. The rock is a tough hornblende gneiss, with veins of pure quartz, and lies in strata of various degrees of inclination. The plan of operations was to build a coffer-dam on the rock near the shore to bar out the water, to sink a shaft to the requisite depth, to honeycomb the whole rocky mass by excavation, and then to blow up the shell by charges of dynamite in the roof and supporting columns, to be fired by the agency of galvanic batteries. The shaft was sunk to a depth of 33 feet below the line of low water, and ten tunnels were then opened to distances varying from 31 to 126 feet. The cubic contents of the rocky mass above the depth of 26 feet at mean low water, amounted to 51,000 yards. The tunnels radiating from the shaft varied from 7 to 22 feet in height, and from 9 to 12 feet in width, and as they advanced the height rapidly decreased, owing to the downward slope of the surface of the reef. As the main tunnels diverged from each other, subsidiary tunnels were introduced, and a system of transverse galleries was excavated, and which left 172 supporting pillars of variable dimensions. The total length of tunnels was 4857 feet, and the length of galleries 2568 feet, making the entire length of passage excavated 7425 feet. The excavations being completed, so that the roof of rock above was reduced to a thickness of from 8 to 16 feet, the preparation for the explosion began by drilling the rock for the charges. The whole number of blast holes drilled into the roof and piers was 4427, varying from 7 to 10 feet in depth, and from 2 to 3 inches in diameter. Each one of these holes was charged with three kinds of explosives,

all composed of nitro-glycerine, viz., dynamite rendrock and vulcan-powder in separate cartridges or canisters. Fifty thousand pounds of these explosives were buried in the apertures. Ninety-six galvanic batteries, of ten cells each, were employed to ignite the charges. The firing point was 650 yards from the shaft, and the amount of leading and connecting wire used to bring all the charges into relation with the batteries was 220,000 feet. The charges in the different holes of the same pier were connected so as to explode simultaneously, but a fuse composed of a quick explosive was used to connect the system of charges in each pier with those of the neighbouring piers. In this way the electric spark taking effect in a few centres, the ignition was propagated through the whole system, as the explosion of the connecting fuse would advance more rapidly than the destruction of the rock. The several thousand charges in the mine were connected in 23 groups of batteries. These were ingeniously connected in a mechanical arrangement so simple and perfect that a child could operate it, and the whole stupendous force that slumbered in the charges was actually released by the touch of a little daughter of General Newton, two years and a half old. The explosion was accompanied by no very stunning effects to eye or ear, and the demonstration was so moderate as to produce great disappointment in the crowds who assembled to witness it. There was a succession of shocks, lasting a few seconds, with no great noise, a mass of water and *débris* of the coffer-dam thrown into the air, and the great reef was shattered and demolished.

TECHNOLOGY.

At the meeting of the Chemical Society on November 2nd, a preliminary notice by Messrs. W. R. Hodgkinson and H. C. Sorby was read, on "*Pigmentum nigrum*, the Black Colouring-matter contained in Hair and Feathers." When perfectly white hair or feathers are heated gently with dilute sulphuric acid for some time they completely dissolve, but if black or brown feathers or hair are thus treated an amorphous black residue is obtained. This substance, which exists only in very small quantity in the blackest feathers, may be conveniently prepared from rooks' feathers (which yield about one per cent) which have been separated from the central rib, and thoroughly cleaned from waxy and fatty matter by treatment with alcoholic ammonia. On digesting them with successive quantities of dilute sulphuric acid for several days, until the acid ceases to be coloured by red or brown soluble colouring-matters, a black residue is obtained, which, after being thoroughly washed with dilute hydrochloric acid at 80° C., and then with water, is dried, and the last trace of fatty matter finally removed by treatment with boiling alcohol and ether. On analysis it gives numbers agreeing very well with the formula $C_{18}H_{16}N_2O_8$. It is not acted on by dilute acids or alkalies, but nitric acid slowly oxidises it. It forms new compounds by the action of bromine, one of which is soluble in water, and gives a characteristic absorption spectrum.

At the Chemical Works at Aalborg, in Jutland, Denmark, where about 30 tons of alkali are made per week by the ammonia process for obtaining alkali from seaweed, Mr. Thowald Schmidt, the Director of the Manufactory, proposes to work, in conjunction with this process, a method devised by himself of treating seaweed so as to obtain iodine, potash salts, and other marketable products therefrom. In Denmark a very heavy duty is levied on the importation of common salt, whilst enormous quantities of seaweed rich in iodine and potash can be obtained at small cost in the neighbourhood of the works. Mr. Schmidt's process is as follows:—After the seaweed is dried and burnt a concentrated solution of the ash is made and added to the liquor containing chlorides of sodium and calcium, left after the ammonia has been recovered in the ammonia-soda process by boiling with lime. The sulphates of potash, soda, and magnesia contained in the ash of the seaweed are thereby decomposed, and hydrated sulphate of lime and hydrated magnesia are precipitated in a form which may be available for paper-making as "pearl-hardening." The last traces of sulphates are got rid of by adding a small quantity of solution of chloride of barium. To the clear solution nitrate of lead is now added

until all the iodine is precipitated as iodide of lead, which is then separated by filtration and treated for the production of iodine or iodides. After filtration the liquid is boiled, nitrate of soda is added to convert the chloride of potassium present into nitrate of potash. The latter is separated by crystallisation. There remains a solution of common salt containing traces of ammonia from the previous soda operation and a trace of chloride of potassium. This solution is again treated by the ordinary ammonia-soda process for the production of bicarbonate of soda and white alkali.

Under the title of "Country Laboratory Apparatus," Mr. Edward T. Hardman, F.C.S., of H.M. Geological Survey, Ireland, describes a substitute for crucible jackets, useful to those who have occasion to shift their quarters often, and are obliged to work with a necessarily limited laboratory accommodation. The ordinary crucible jacket being made of sheet-iron has in reality but one use—to protect the flame from currents of air. An ordinary earthenware flower-pot answers the purpose in every respect. It is the proper shape,

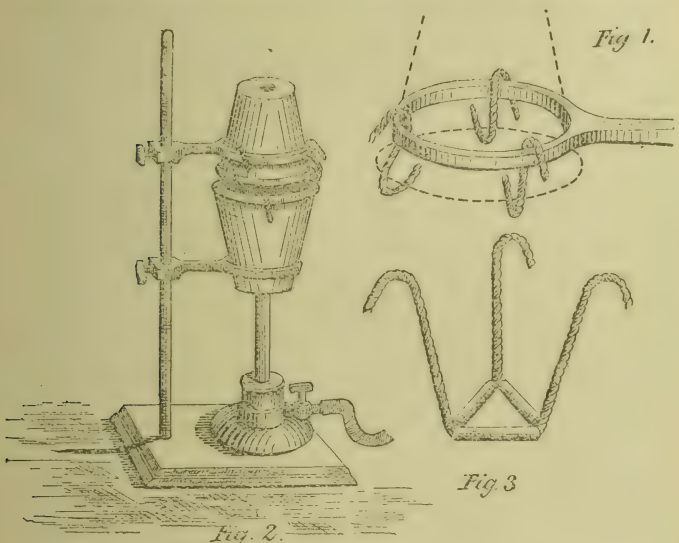

Fig. 1.

Fig. 3

Fig. 2.

and being made of a non-conducting material it in a great measure prevents loss of heat from the burner. The bottom of the flower-pot has a circular hole: this serves for the introduction of the Bunsen burner. As the supply of air would be otherwise insufficient, it will be necessary to enlarge the opening. This can be easily done by cutting the aperture nearly in the form of a cross, and not too large. A current of air is then obtained which not only steadies the flame, but acts in some degree as a blast. The flower-pot may be supported in the ring of a retort-stand in the usual way. The chimney is a second flower-pot inverted. To support it the handiest way will be to make three S hooks of stout wire, and having passed the narrow end of the pot upward through the ring, fix the rim within the hooks caught on the ring, as in Fig. 1. The apparatus acts admirably as a small gas furnace for crucible operations, such as the fusion of silicates with carbonate of soda—as in the analyses of rocks; while for simple ignition of precipitates it renders the flame of a common glass spirit-lamp most effective. The size of the flower-pot required will, of course, depend on that of the crucible and of the burner used. The support for the crucible may be either a triangle of wire covered with

pipe-shank, the end of the wire being bent upwards and formed into hooks so as to hang on the edge of the flower-pot (Fig. 3), or three pipe-covered wires, suspended in the position of the ribs of a crucible jacket. The former is necessary for small crucibles. The flower-pot also makes an excellent lamp-screen, for steadying and concentrating the flame under evaporating basins, &c. A small flower-pot with wire gauze tied over the top is a very effective low temperature lamp when the gas is lighted before the gauze. If the gas is lighted above the gauze an argand lamp giving a large clear blue flame is obtained. In the latter case a common burner can be used. Having occasion to determine the volatile matter of a coal, and not having at hand the usual elaborate arrangements, Mr. Hardman thought of the schoolboy's method of manufacturing coal-gas. The retort he uses is a common clay tobacco-pipe. A piece of coal is put in, the top is luted with clay, and the pipe is inserted in the fire-grate with the stem projecting. Presently a dense smoke issues from it, and and a match being applied a veritable gas light—but not " 16-candle " —results. On opening the luting a piece of coke is found in the pipe. It is obvious that it is only necessary to weigh the pipe and contents before and after the operation, and we have the volatile matter and coke determined. The larger the pipe the better. The coal must be broken small, but not powdered. The pipe is weighed, then filled with the coal aud weighed again to obtain weight of coal. Then inside the top is fitted a circular piece of writing-paper, the use of which is to prevent any of the luting getting down among the coal, where it could not be removed, and would falsify the last weighing. The top is luted with moist fireclay, or with the cement used for luting the covers of gas retorts, and the pipe being placed in a common coal fire or in a gas furnace from ten to twenty minutes completes the operation. When cool the luting is carefully taken off and the charred paper removed. The pipe and contents being then weighed, the loss gives the volatile matter, the same weighing, of course, determining the coke. If a very exact determination is required, a quantity of the coal may be broken small, well mixed together, and four pipes filled as above. They can all be ignited together in a fire, and weighed very quickly. The results will be found to agree very closely. This tobacco-pipe process has the great advantage of being very expeditiously performed—the whole experiment including weighings not occupying more than thirty minutes—and with very simple apparatus.

THE QUARTERLY

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I. THE BALANCE OF NATURE.*

WHAT naturalist does not look back to his first perusal of Waterton's works as one of the most agreeable reminiscences of his boyhood? Even now, notwithstanding the wonderful transformation which Natural History has undergone, the "Wanderings" and the "Essays" retain their old charm unimpaired. If we attempt to analyse the merits of Waterton we cannot, indeed, regard him as a systematist. His chief attempt in that direction, the arrangement of the Quadrumana, formerly so called, into four groups,—the tail-less, or apes; the short-tailed, or baboons; the ordinary long-tailed monkeys of both hemispheres; and, lastly, the prehensile-tailed species of South America,—might almost be regarded as a satire on the classifications of some of the older naturalists based upon a consideration of some single organ. If taken seriously, it has the striking demerit of breaking up the platyrrhine family into two groups, in defiance alike of morphological and of geographical considerations. But if unable or unwilling to inaugurate a "system" of his own, Waterton's clear incisive common sense refused to accept the Swainsonian doctrines, which reigned supreme during many years of his life. "Who knows," says he, "but that some closet-naturalist may account for these alar spurs of the camichi through the medium of that very useful and important discovery, the quinary system. Thus, for example, suppose these said spurs were once normal or typical on the legs; but by some rather obscure process, having become aberrant, they

* Game Preservers and Bird Preservers. By G. F. MORANT. London: Longmans and Co. Essays on Natural History. By CHARLES WATERTON. Edited by NORMAN MOORE, B.A. London: F. Warne and Co.

made an approach or passage to the wings; whilst the bird itself was progressing in the circle or leading round, in order to inosculate with the posteriors of its antecedent. He who clearly understands the quinary system will readily understand this." Scientific nomenclature he avoided, and even denounced as abstruse, complicated, and incomprehensible—a view likely enough to be taken up by one of the working-class naturalists who used to be met with in the North, but strange and scarcely consistent in a gentleman who had received a classical education, and who was rather too much given to Latin quotations. Had he lived in our days this prejudice for a vernacular terminology might have been a pardonable and even useful protest. Generalisation was not his department. Those questions concerning the origin, the modifications, and the distribution of species, which are agitating the zoologists and botanists of our time, and which must be definitely answered before Natural History can be formally ranked as a Science, do not appear ever to have presented themselves to his mind. He accepted every bird and beast as an ultimate fact; he scrutinised its structure, noted its habits, but never enquired why it was found in one country and not in another, nor what were its relations to other species, co-existent or extinct. Neither can Waterton be regarded as free from prejudices. He stoutly upholds the conventional doctrine of a great gulf between man and the lower animals—a distinction not of degree, but of kind. He denies reason to dogs, to foxes, to apes, and, in short, to all the lower animals, and regards it as a characteristic of man alone. All instances of rationality on the part of bird or beast he regards with an excessive scepticism, somewhat singular in a man who could record his firm belief in the miraculous liquefaction of the blood of St. Januarius, at Naples. He entertained the view that no carnivorous animal could be gregarious, and hence was led to deny that wolves and other Canidæ were naturally in the habit of hunting in packs, and of acting in concert to secure their prey. It is needless to say that had he ever visited Russia, or the more northern parts of North America, he would soon have found overwhelming evidence to the contrary, and might easily have been convinced of his mistake by his own observations. In addition to the dog tribe, the common weasel has been known to run in packs, and men have occasionally been hard set to escape with their lives from the pertinacious attack of a band of these little vermin.

Waterton also denies that serpents, unless trodden upon

or intercepted in making their escape, ever act upon the offensive towards man. In his extensive experience he had doubtless never met with such a case. Our own observations agree in this respect with his, but for all this we are not prepared to deny that the ophiophagous cobra—a specimen of which is now to be seen alive in the Zoological Gardens—will, under certain circumstances, attack passers-by. Waterton's great error was a readiness to assume that what he had never seen, or perhaps never had had the opportunity of seeing, must be imaginary. But if, as we have good reason to believe, he is wrong in this particular, he has made ample amends by exposing and refuting a multitude of idle stories about serpents which are related even in books of some pretensions to scientific accuracy. As regards our English serpents, Waterton seems to apply the name of "adder" to the common "water-snake" (*Natrix torquata*), but he underrates the venom of the viper: death has undoubtedly resulted from its bite, even in England.

As a controversialist Waterton was truly formidable, if somewhat too outspoken for modern tastes. Any error in the writings of an opponent he was sure to detect, and to expose without mercy. Witness his treatment of Audubon's rattlesnake, figured with its poison-fangs curved the wrong way. Upon pretentious dabblers in Natural History, who seek to force themselves into notoriety by retailing the results and sometimes the blunders of others, he was severe. He "scourged two generations of quacks," and we sometimes think he might do good service, even in the present day, could he be roused up from his slumbers beneath the old oaks in Walton Park.

We never heard of his being connected with any scientific society; perhaps he might feel unwilling to place himself upon a level with "Diabolus Gander," and others of his contemporaries who were Fellows of every existing society, and would doubtless have been "Fellows" of Vauxhall and Ranelagh had those establishments been original enough to assume a semi-scientific disguise. His paramount merit is as an observer of phenomena, and in this department he has had few equals and no superior. In order to see with his own eyes he deemed no trouble or danger too great, and whatsoever he saw he describes faithfully and clearly, overlooking nothing and exaggerating nothing. The great misfortune is that all his writings have not seen the light, and that he committed to paper not one-tenth part of the facts that he observed. He has left us, as it were, merely a sample of his labours.

It is not, however, our present purpose to enter upon an exhaustive appreciation of Waterton as a naturalist, or to decide upon his ultimate position in the history of science. We have to consider his views upon a subject which has embarrassed naturalists and teleologists almost as much as the "balance of power" has perplexed politicians. Is there a balance of Nature? If so, in what does it consist, what does it signify, and in how far can it be modified to our advantage? These are the questions we have to consider, and it will be at once conceded that they not merely concern the zoologist or the botanist in his study, but come home more or less to us all in the concerns of daily life.

If we examine some island, as yet untrodden or rarely visited by man, and take a broad view of its fauna and flora, we must admit that a certain balance exists. The species there found, by the very fact of their co-existence, are proved to be mutually compatible. The carnivorous mammal, bird, or insect does of course devour a number of its feebler fellow-denizens of the island: the phytophagous mammal, bird, or insect, in like manner, preys upon the plants. But the greater fecundity of the species preyed upon, or some other attribute, preserves them from extirpation, and even prevents any appreciable decrease in their numbers. The equilibrium may not, indeed, be absolutely perfect. Could we visit such a region at successive periods of a century each, and take an exact census of its population, animal or vegetable, we might find this or the other species growing gradually rarer, and finally becoming altogether extinct, whilst other species, on the contrary, were slowly but surely increasing. Such changes may arise not merely from the ravages of the destroyer slowly gaining ground upon the fecundity of some one kind of his prey, but still more decidedly from changes of climate. However, within moderate periods of time, the equilibrium in any country may be regarded as complete until the intervention of man.

Recognising, then, a certain phenomenon which may be called the balance of Nature, we have to consider its origin and meaning. On this subject there exist various theories pointing the way to diversities in practice.

The view formerly most general may be considered a corollary to the Miltonic interpretation of the Mosaic cosmogony.* It regards the fauna and flora of every region, as especially selected by Providence, as perfectly adapted

* It is significant that Prof. Huxley should be accused of insulting the understanding of the American people by having referred to this poetical interpretation as still accepted in certain quarters.

both to each other and to the soil and the climate of their locality. Writers holding this opinion have even likened the various animal and vegetable species to the wheels and pinions of some delicate and complicated piece of machinery, none of which can be removed without injury to the whole. Man's true policy, therefore, is to let Nature alone. The more he interferes the less reason will he have to be satisfied with the results. It must be owned that far too many facts lend plausibility to this view. In nothing has man shown a more glaring want of prudence than in the wars he has waged against the lower animals; but he cannot, however willing, leave the balance of Nature undisturbed: he is compelled, by the law of self-preservation, to extirpate such carnivorous, venomous, parasitical, or otherwise destructive species as are dangerous to himself, his cattle, or his crops. Singularly enough, his appliances for waging war against such creatures have been developed far less rapidly than his means for the destruction of his fellow-men. But this is not all: he brings with him, in some cases by design and in others unintentionally, animals and plants which soon exert a powerful modifying influence. He is almost invariably accompanied by the grey rat, which at once goes to work to revolutionise the indigenous fauna. The small birds which have hitherto built in security on the ground, or in low trees, have their eggs and young carried off and devoured; the lizards and the terrestrial Mollusca are attacked on the land, and the smaller fishes in the streams. Apterous insects, and the pupæ and larvæ of winged kinds, are sought for in all their haunts by the energetic destroyer; and entire species are thus erased from the muster-roll of Nature. Such is the inevitable result where the rat is kept in check neither by serpents, birds of prey, nor carnivorous beasts. As an instance we may take Mauritius, where rats have multiplied to a frightful extent, and where the present poverty of the fauna, especially in insects, is notorious. But the animals which man introduces intentionally, and for his own use, are perhaps equally destructive. Foremost in this respect stands the goat; he browses away the shrubs and the young seedling trees, and thus ultimately extirpates the woods. With the trees perish the insects; and with the insects, fruits, and seeds the birds disappear also: the destruction of the aboriginal vegetation of St. Helena—a loss deeply felt by all students of organic geography—must be, in the main, ascribed to the ravages of goats. Swine take a prominent part in the work of destruction, and are particularly busy in eradicating all vegetables with bulbous or tuberous roots, or

with succulent stems and leaves. Scarcely less formidable is the rabbit, which, when thoroughly established in a region free from beasts and birds of prey, renders the introduction of agriculture almost impossible. To naturalists, at least, it is a subject of grave regret that a project has been brought forward for stocking uninhabited islands with goats and rabbits, under the pretext of thus providing a supply of food for shipwrecked sailors. The result of this scheme, if carried into execution, will be the destruction of much priceless evidence bearing on the distribution of animal and vegetable life, and hence indirectly on the great question of the origin of species.

But worse still remains: the islands of New Zealand are suffering under a grievous plague of rabbits. It has been suggested by an "energetic and indefatigable naturalist" to introduce, by way of a remedy, polecats! Suppose this done, it is very probable indeed that much havoc may take place among the rabbits; but is it not likely that the poultry of the colonists may receive certain unpleasant attentions, and that some of the few native birds will be exterminated? Nor is the action of the animals imported by man the only agent of change. The aboriginal plants, contrary to the theory of their especial and exceptional adaptation to the soil and the climate, largely succumb to the species introduced by the settlers, many of which, unfortunately, are weeds of the most noxious character. Thus we see that the arrival of man in a previously unpeopled region completely disturbs its original "balance of Nature," even if he never discharges a gun. But as, in addition to the havoc thus indirectly occasioned, birds good for food, or suspected—rightly or wrongly—of being injurious to crops, are diligently shot at, we need not wonder that the old equilibrium is speedily overturned. A different theory is therefore forced upon all candid thinkers. Whether they are believers in original and independent creation, or in evolution, they must equally admit that the old "balance of Nature" is merely provisional, suited to man's absence, but incapable of being maintained in his presence. A new equilibrium has therefore, if possible, to be established, where all species of animals and plants dangerous or hurtful to man's person and possessions shall be exterminated, or at least very much reduced in numbers, whilst those which are useful, beautiful, or otherwise agreeable to the lords of the creation, shall be protected and encouraged. That such a state of things would be theoretically desirable we shall all agree, but how is it to be realised in practice? Too many farmers,

colonists, &c., destroy the wild birds upon very insufficient evidence, and then make, too late, the unpleasant discovery that they have been extirpating their best friends. Thus, as Waterton tells us, "the North-American colonists got the notion into their heads that the purple grakle was a great consumer of their maize, and these wise men of the west actually offered a reward of threepence for the killed dozen of the plunderers. This tempting boon soon caused the country to be thinned of grakles, and then myriads of insects appeared, to put the good people in mind of the former plagues of Egypt. They damaged the grass to such a fearful extent that in 1749 the rash colonists were obliged to procure hay from Pennsylvania, and even from England. Buffon mentions that grakles were brought from India to Bourbon to exterminate the grasshoppers. The colonists, seeing these birds busy in the new-sown fields, fancied that they were searching for grain, and instantly gave the alarm. The poor grakles were proscribed by Government, and in two hours after the sentence was passed not a grakle remained in the island. The grasshoppers again got the ascendancy, and then the deluded islanders began to mourn for the loss of their grakles. The governor procured four of these birds from India, about eight years after their proscription, and the State took charge of their preservation."

The random destruction of wild birds has occasioned most serious results in France. Till a quite recent date every species of bird in that country was subject to persecution. The robin and the swallow were shot and eaten as eagerly as the quail or the ortolan. To make matters worse, our contingent of swallows take the route through France on their southward flight in the autumn, and are too often intercepted on their way. The Italians, in like manner, prey upon birds of passage, to the great detriment of the German and Swiss farmers. It is even intimated that negociations on this subject have been begun between the German and the Italian Governments. The consequence of this reckless assassination of small insectivorous birds has been a frightful increase of caterpillars, aphides, cockchafers, weevils, and other insect scourges of the farm and the garden. Worse still, blood-sucking flies have multiplied, whose bite is sometimes followed by carbuncle, and even proves mortal—possibly in cases when the fly has been previously feasting upon carrion in some particular stage of decomposition. The French have therefore perceived the error of their ways. Not only have laws been enacted to restrain the murderous propensities of misguided sportsmen,

but official notices have been posted up throughout the country, by order of the Minister of Agriculture, pointing out what birds, insects, &c., are injurious and should be destroyed, and what are useful and should be spared.

Perhaps Waterton's highest claim upon our esteem and gratitude must be based upon his energetic protest against such indiscriminate slaughter as we have just mentioned. Though a most humane and kindly man, he spoke not as a hysterical humanitarian, ever seeking to protect evil-doers, whether brute or human, rats or garotters, from the well-merited consequences of their misdeeds. His protest was uttered in the name of common sense no less than in that of compassion. He had, during a long and active life, devoted almost exclusively and under exceptionally favourable circumstances to observation, carefully examined the structure and the habits of birds and of certain mammals, and had noted the kinds of food which they select. His park was simply an ornithological "station," where all kinds of British birds were as far as possible protected, in order that they might be closely and accurately studied. Hence he was enabled to pronounce with authority as to what species were to be viewed as man's friends, and which were to be ranked as enemies. Not a few more recent British naturalists—among whom honourable mention is due to the Rev. Messrs. F. O. Morris and Tristram—have followed in his footsteps; and if our Legislature has been induced to extend any measure of protection to wild birds not in the game-list, to Waterton and to his disciples belongs the credit.

If asked, however, to describe in a few words Waterton's system of establishing a "balance of Nature" suitable for inhabited countries, we should reply that he sought to minimise the amount of man's interference. He would not sign the death-warrant of any creature till positively proved to be injurious; and if the evidence was imperfect, then, in accordance with the old-established practice of English justice, but in flat opposition to the custom of English gamekeepers, he would give the accused the benefit of the doubt. More than this, if—an exceedingly common case—any species were found to be in some respects harmful, but beneficial in others, he would endeavour to balance the evil against the good. Here, of course, there is room for a diversity of opinion among men of different views, interests, and prejudices. Suppose, for instance, that some particular bird frees our fields from millions of destructive insects, but at the same time occasionally carries off the eggs of the

pheasant or the partridge; what is to be the verdict? One man—thinking the food of the majority of the nation and the prosperity of our farmers a matter of greater importance than the production of costly delicacies, and than the amusements of a small minority—may vote for an acquittal. Others, such as Mr. Morant, may take the opposite view. It is certain, indeed, that Waterton may have been somewhat too lenient, and have overshot the mark, in his general protection of birds. It is the common, perhaps the inevitable, error of the reformer combatting some wide-spread and old-established error, that he goes too far; but he is doubtless much nearer the truth than any of his gainsayers, past or present.

The strangest view of man's proper policy towards the lower animals has been put forward by Mr. Morant. This author has certainly some points of resemblance with Waterton. He has travelled extensively; he has "pursued and collected birds over a great part of India, and for some years in South Africa." Hence he may, perhaps, be regarded as a compound of sportsman and naturalist; the latter, however, in a somewhat homœopathic proportion. He is also a teleologist. Thus he tells us that—"Most animals' powers of reproduction are so great, *evidently with a view of directly or indirectly affording food to man*, that but for some such check* before the appearance of man upon the scene they would have crowded each other out, and have died miserably of starvation." Without any further examination of the logic of this passage we will call attention to the words we have italicised. Amazing fecundity is certainly no especial attribute of species which contribute to the support of man. It is possessed to a fearful degree by the rat and the mouse, the locust and mole-cricket, the crane-fly, the ant, the aphis, the turnip-fly, the phylloxera, and the Colorado potato-beetle. Were anyone disposed to take a pessimist view of creation, and give a theory of the rapid increase of animals exactly opposite to that of Mr. Morant, he could not be easily refuted. Like Waterton, our author is fully convinced of the injury done to our fields and gardens by the ravages of insects. In support of this view—which, indeed, no competent authority will for a moment feel inclined to question—he appeals to the evidence given before a certain "Select Committee appointed in 1873 to inquire into the advisability of extending the protection of a close season to certain wild birds not included in the Wild Birds'

* *I.e.*, the existence of carnivorous creatures.

Protection Act of 1872." Before this Select Committee appeared Mr. Groome Napier, and gave, as quoted by Mr. Morant, the following most extraordinary evidence :—" The beetles (Coleoptera) are immensely numerous as regards species. They live from three to four years in the larvæ state. The first year they do not do a great deal of damage. The second year they attack the roots of all plants within their reach ; they often ruin the crops of corn, lucerne, &c., on which man depends for food." We can only hope that some qualifying clause has been overlooked by Mr. Morant. Surely both he and Mr. Groome Napier ought to know that, numerous as are the beetles in species, multitudes of them, such as the common ground-beetles (Cicindelidæ, Carabidæ, &c.) and the water-beetles, are, in all stages of their existence, purely carnivorous. The larvæ of others, such as the dung-beetles (Geotrupidæ and their allies), feed upon the dung of animals buried in the ground by the parent insect as a supply for its offspring. Others, again, both in their preparatory stage and when mature, nourish themselves upon putrid animal matter. The larvæ of the long-horns and Buprestids feed on timber, and the weevils—noxious as many of them are—in the buds and fruits of trees. In short, it may be safely maintained that not one-tenth of the species of Coleoptera destroy the roots of plants ; and Mr. Groome Napier, if he has really used the language here quoted, without any saving clause, grievously slanders a multitude of beings, many of which are the most faithful allies of the farmer and gardener.* We think that Charles Waterton would neither have written such a passage nor quoted it, excepting with the intention of inflicting a severe castigation upon its author. But whilst Mr. Morant agrees with the worthy Squire of Walton Hall as to the importance of small birds in rurul economy, he dissents from him altogether as to the causes of their decrease and as to the means to be taken for their preservation. Waterton considers that the worst enemy of the feathered tribes is man, and especially that variety of man known in modern England as gamekeepers. These he pronounces to be " privileged scourges of animated nature," " unrelenting butchers of our finest and rarest British birds." Mr. Morant,

* Not being quite sure at the moment whether Mr. Groome Napier was to be regarded as a naturalist or as a sportsman, we turned to our General Index, and found that he is the author of a work entitled the " Book of Nature and of Man," from which we extract the following passage :—" They (lichens) have a strong resemblance to cancers (*Morbus Brightii*), if they are not to be classed together." Cancer a synonym for *Morbus Brightii !*

on the contrary, maintains that—"It is a great mistake to suppose that man drives away birds by cultivating the surface of the earth. He feeds hundreds in feeding himself, and they are infinitely more numerous in our gardens and on our farms than in the primeval forest where his foot never penetrates, or on the great fertile plains where he has not yet turned the earth." To this passage we shall have occasion to return. He seems indignant at the statement of naturalists that "gamekeepers are ignorant and cruel, and do more harm than good." He asserts that "the sorts of birds they (gamekeepers) kill can be counted on their fingers and their numbers in scores, whilst the sorts of birds they protect are counted by hundreds and their numbers by tens of thousands." The case mentioned by Mr. Stevenson before the Committee, of a Norfolk gamekeeper who said that he shot the nightingales and took their eggs lest they should disturb his pheasants in the night, Mr. Morant disposes of by the clever surmise that the keeper might be amusing himself at the expense of his listener!

Whom, then, ought we to believe? Mr. Morant and the "game-preservers," or Waterton and the "bird-preservers?" Now though, as we have already mentioned, we have little faith in Waterton in matters of inference or of generalisation, in a question of direct observation we doubt if he has ever been found mistaken. He was himself a country gentleman and a sportsman, and certainly would not have denounced the gamekeepers as he did without sufficient reason. Our own observations fully confirm what he has advanced. Many a time have we grieved to see not merely hawks, magpies, crows, and jays, but owls, woodpeckers, goatsuckers, and birds of many other kinds, nailed up against the gable end of a keeper's lodge. Of our own knowledge we endorse the remark of one of the gentlemen examined before the Committee, that "an average gamekeeper kills everything as vermin except what is in the game list." They suspect all animated nature of harbouring designs against their pheasants or "birds," and where there is the shadow of a doubt they act as if there was convictive evidence. An enlightened employer may sometimes attempt to restrain their mischievous zeal, but behind his back the havoc will go on. We have lately read an instance of a keeper who, when rebuked by his master for having shot a goatsucker, and told that the bird was perfectly harmless, replied "Well, sir, but it's a narsty flopping thing."* As

* *See* Science Gossip, iii., 17, and v., 179.

to ignorance, Mr. Morant himself quotes, without any expression of scepticism, the story "of a gamekeeper who told the Rev. Mr. Tristram that 'he was sure the cuckoos turned into hawks in the winter, for if not, what became of them?'" We have ourselves heard German gamekeepers —who are, to say the least, quite as intelligent and well-informed as their English brethren—narrate the most frightful and grotesque stories concerning vipers, salamanders, and newts. But we shall easily understand what must be the behaviour of the gamekeepers towards birds and beasts by a reference to their conduct to men. As they classify all the lower animals under two heads, game and "varmint," so they view all mankind either as game-preservers or as poachers. A botanist or entomologist, however strictly he may abstain from any act of trespass, is always in danger of annoyance and insult in districts where keepers are rampant; and, in a manner perfectly analogous, whatsoever bird or beast is to them strange or novel, that they destroy.* But how are we to get over the evidence of Mr. Morant, who flatly asserts the contrary, and who, mistaken as we hold him to be, is evidently sincere? Let us examine, in the first place, his point of view. His method of re-adjusting the disturbed balance of Nature we have already pronounced strange. He tells us, substantially, that if we will only preserve game by a diligent application of the system now in vogue, and especially by shooting down all hawks, jays, crows, and ravens, the end will be gained. The small birds, whose services we require to rid the land from insect pests, will be benefitted quite as much as the game. If Mr. Morant is in the right the game-preserver must be regarded as a public benefactor. The plea, it must be admitted, is highly ingenious, but we doubt if it can be pronounced valid. He sometimes forgets his zeal for the small birds. Thus, speaking of the black grouse, he exclaims—"Not a naturalist has a word to say for him, while the disappearance of such birds as the siskin or garden warbler is constantly regretted." Naturalists might reply that—leaving game birds to the protection of their numerous and influential *friends*, the game-preservers—they plead for those that have hitherto had no protector. At times, too, if not actually inconsistent, he acts very decidedly up to the

* To prevent any misunderstanding, we may state that we have not the least sympathy for the poacher. He very frequently graduates higher in the school of crime, and becomes a burglar, a sheep-stealer, perhaps a garotter: at the best he is but a gamekeeper *in opposition*. The two are respectively convertible, just as are the conspirator and the *mouchard*, or the martyr and the persecutor.

maxim that circumstances alter cases. Thus in most parts of Britain he would extirpate eagles, hawks, and ravens, in the most thoroughgoing style. He describes, in a manner at once graphic and pathetic, the "sufferings of the young grouse when their mothers are taken,"—the "screams and helpless terror of the old birds, and the feeble efforts of the little ones to escape." But in a "deer-forest"—say rather in a "deer-desert"—he would protect golden eagles to keep down the hares, and falcons to kill the grouse! Yet he questions the sincerity, or at least the consistency, of those who "pretend to care for birds, and yet harbour and encourage cats." Were Waterton's system of tolerating the carrion crow to become general, he maintains that it would be impossible to rear chickens or ducklings except under a net. Yet he recommends the preservation of the fox, and counsels farmers to guard their poultry against his depredations by means of wire net-work. In mountainous regions, however, the fox is to be shot, trapped, or poisoned, as most convenient. Rabbits are to be "ferretted down" till there are only enough left to serve as food for the foxes. He thinks that eggs require fresh air and warmth, and that there is "*nothing more unpleasant than the smell of wild flowers round a pheasant's nest.*" So the very flowers, we presume, are to be extirpated! It is hard to trace, in these recommendations, either humanity, or a desire to promote the interests of agriculture, or the wish to protect useful and beautiful beings, or to establish a new and improved "balance of Nature," or anything but the love of "sport."

Then the small insectivorous birds, whom we desire to protect, have other enemies beside those enumerated by Mr. Morant. The shrikes make sad havoc among them at times; but these minor birds of prey are utterly unable to overpower a partridge, a pheasant, or a grouse, and—shall we say *therefore*—Mr. Morant passes them by undenounced. There is very considerable suspicion, if not positive proof, that the squirrel at times makes free with the eggs and the young of small birds, but he, too, escapes our author's censure.*

We have further to ask why, if—as Mr. Morant seems to hold—hawks and crows are the great obstacle to the increase of beautiful and useful birds, these latter are found to decrease, *pari passu*, with their supposed destroyers? Yet this is undoubtedly the fact. England is decidedly growing poorer in species of birds. Kinds once common are becoming rare; those formerly rare are in many parts verging

* The squirrel undoubtedly destroys pears, peaches, and plums, and even the blossoms of the cherry,

upon extermination. But is it not almost preposterous to ascribe this change to the depredations of species which are themselves on the wane? Should we attribute, *e.g.*, a decrease of game in any district to poachers if these worthies were all the while becoming scarcer and scarcer?

The causes to which we should ascribe the decrease of ornamental and useful birds are many, but they may all be traced to man rather than to hawks, eagles, or crows. Foremost comes, as we have already maintained, the gamekeeper. Mr. Morant, in seeking to deny that this "assassin in velveteen" shoots down anything but predatory birds, reminds us of Waterton when maintaining that serpents never act on the aggressive. Each of them is trying to prove a negative which one affirmative instance to the contrary must at once overturn. Mr. Morant, when surveying Nature, seems to have a pheasant's egg before one eye and that of a partridge before the other, and views all phenomena through this very perplexing medium.

We might even ask, with Waterton, whether the very game birds themselves do not suffer quite as much from their official protectors as from their natural enemies? In his Essay on the Carrion Crow, Waterton remarks—"This man probably never reflects that in his rambles to find the nests of the birds he has made a track which will often be followed up by the cat, the fox, and the weasel, to the direful cost of the sitting birds; and moreover, that by his own obtrusive and unexpected presence in a place which ought to be free from every kind of inspection, whether of man or beast, he has driven the bird precipitately from her nest, by which means the eggs remain uncovered. Now the carrion crow, sweeping up and down in quest of food, takes advantage of this enforced absence of the bird from her uncovered eggs, and pounces upon them. Had there been no officious prying on the part of the keeper, it is very probable that the game would have hatched its brood in safety, even in the immediate vicinity of the carrion crow's nest,—for instinct never fails to teach the sitting bird what to do. Thus in the wild state, when wearied nature calls for relaxation, the pheasant first covers her eggs, and then takes wing directly without running from the nest. I once witnessed this, and concluded that it was a general thing. From my sitting-room in the attic storey of the house I saw a pheasant fly from her nest in the grass, and on her return she kept on wing till she dropped down upon it. By this instinctive precaution of rising immediately from the nest on the bird's departure, and dropping on it when returning, there is

neither scent produced nor track made in the immediate neighbourhood, by which an enemy might have a clue to find it out and rob it of its treasure."

"Keepers may boast of their prowess in setting traps (and in testimony of their success they may nail up the dead bodies of carrion crows against the kennel wall); but I am of opinion that if the Squire could ever get to know the real number of pheasants and hares which have been killed or mutilated in those traps, he would soon perceive that he had been duped by the gamekeeper. The frequent discharge, too, of the keeper's gun, though it may now and then kill or wound a carrion crow, still will infallibly drive away the game in the end, and oblige it to seek some more favoured and sequestered spot." This is cogent reasoning, and we do not see that it is anywhere fully met by Mr. Morant. The disturbing effect of the discharge of guns he himself admits. He tells us that in very remote parts of Scotland he found the grouse "as tame as the boobies which the sailors knock on the head in the South Sea Islands." They actually declined to fly up and be shot in the only orthodox manner, and required to be gradually educated into shyness.

Secondly, as a cause of the increasing scarcity of many interesting birds, we accuse not the sportsman,* in the ordinary English sense of the word, but the troops of roughs who on public holidays sally forth from our towns into the surrounding country and fire promiscuously at everything having wings and feathers, no matter on whose property. If we really wish to protect birds we must put severe restrictions upon this class; they are worse than either gamekeepers or poachers.

Thirdly, we must blame the bird-catchers. Scarcely a native bird having any beauty in its plumage or any sweetness in its song can escape the attentions of these marauders; it is trapped, carried away a prisoner, and generally dies at no very distant date, from want of care and from improper food. The wild birds are indeed in the enjoyment of a nominal "close time." Bird-catchers are prohibited from plying their vocation during the breeding season, and nest-robbing is of course made altogether illegal. But the protection is merely nominal; it is not the duty of any person to see that the Act is duly enforced. The

* We should have nothing against the sportsman if he would only do two things:—first, eschew anti-vivisectionism; and, secondly, make better use of his splendid opportunities for the study of animated nature. Too often, however, he regards birds as mere moving targets.

inhabitants of the outskirts of London and of the suburban villages may see, every Sunday morning during the so-called "close season," troops of bird-catchers going forth to their vocation, and returning about 10 to 12 A.M., the cage holding their prisoners being covered over with cloth, so that no one may be compelled to see how the law is being broken. As for the police, they are too busy watching the movements of the *bona fide* traveller ever to ask an inconvenient question as to the contents of those mysterious packages slung over the shoulders of the unwashed depredators. Would it not be possible to form an association for the enforcement of the law?

Lastly, we must point out, as a cause of the decrease of our beautiful and useful birds, the destruction of the woods, and the removal of the old hedges and hedgerow trees, which once redeemed English rural scenery from the "mad monotony" of the cultivated lands in most parts of the Continent. Here we have again to differ from Mr. Morant, who thinks it a mistake to imagine that man drives away birds by clearing and cultivating the earth. He thus falls into that most dangerous kind of error—a half truth.

In the depths of the primæval forests neither birds nor insects are very numerous, as Messrs. Bates and Wallace take occasion to point out, and as we have in former days learned to our cost. It is along the margin of a clearing, at the edge of the lake, the river, or the savanna, or where the woodland meets the cultivated land, that both species and individuals are most numerous. But if you clear the forest entirely away, and convert the cultivated land into a treeless, bushless waste, divided merely by wire fences, or by low thin lines of stubbed thorn hedges, incapable of sheltering the smallest bird, then you get rid of most insects, save those that prey upon your crops, and of the birds that would have kept the latter in due check. This denudation of the country has been tried in Spain, and the consequence is that the land is desolated with grasshoppers, and that forests are being now planted at national expense in order to afford shelter to insectivorous birds. The system of enormous fields divided merely by rail or wire fences has been largely adopted in the Western States of America, and from those States come heavy and well-founded complaints of the ravages of grasshoppers and vermin in general. Small birds cannot be expected to fly from 3 to 4 miles for every caterpillar or worm they carry home to their nestlings. If we wish to preserve them, and to secure the benefit of their services, we must see to it that there is suitable cover

in abundance, and within a reasonable distance of the lands they are expected to clear of insect depredators; otherwise —were every hawk, raven, or other carnivorous bird hunted down and done away with—our finches and warblers would still be few. We will even venture the opinion that were our hedges and hedgerow trees restored to their old condition, and were human depredators put under wholesome restraint, we might have a better supply of small birds than at present, even if hawks, magpies, and jays were less severely dealt with.

We will now take a brief glance at the birds and beasts of prey whom Mr. Morant "tries for their lives," and, with one exception, condemns. He shows great zeal in overruling any plea that may be urged in favour of the accused, and goes on the principle that no amount of benefit conferred upon mankind can atone for any offence against game. Though neither hostile to game-preservers nor desirous of the extirpation of pheasants, we shall not admit this doctrine.

The golden eagle we cannot defend: he certainly carries off lambs to a serious extent, and may attack children. Against the sea-eagle nothing is proved.* The buzzard, which follows next on Mr. Morant's list, may certainly now and then secure a grouse, but he is too slow on the wing to be a successful bird-hunter. On the Continent he is known as a devourer of mice, rats, and snakes, which latter he attacks with much skill and judgment. The hen harrier is next examined, and condemned as a great destroyer of grouse. It is admitted, indeed, that the crop of an old male, when shot, was found to be full of wireworms. Still we fear that the balance of evidence is against him. There are other destroyers of wireworms, quite as efficient and more trustworthy.

The falcon is unquestionably a bird-devourer who rarely partakes of any other prey; nor do we know of any plea that can be urged in his favour. Little, if any, better is the case of the sparrowhawk, unless he occasionally varies his diet with a mouse. Further and closer observation would therefore here be desirable. The kestrel, or windhover, receives a half-grudging acquittal:—"When mice are plentiful," says Mr. Morant, "he seldom takes birds; but he will not starve, and he well knows that the little newly-

* By the way how many sheep does the "useful dog"—as Mr. Morant calls him—destroy in a year? In the State of Georgia some 28,000 per annum! We may thank the dog-tax that the damage in Britain, though very serious, does not reach so appalling an amount.

hatched pheasants and partridges are not bad eating as a change." The gamekeepers, who have "nearly all at some time or another shot him in the act" of bird-killing, have not impossibly confounded him with the sparrowhawk. Waterton, who had many kestrels in his park, observed that the small birds never seemed alarmed at the approach of the kestrel, whilst there was great and general consternation if a sparrowhawk came in sight. Nor did an examination of their nests reveal feathers or other proofs that birds had been brought for the food of the young. Still Waterton admits, on the evidence of his friend Mr. Bury, that the kestrel "will occasionally make a meal on the smaller birds." Granting this fact, we yet maintain that as one of the best destroyers of mice his services far outweigh his demerits, and sound policy demands his preservation.

The owl, too, receives a like kind of doubtful acquittal, and is at any rate pronounced "the very best mouse-destroyer we have,"—which is certainly true.

In the raven we can see no redeeming features. Not merely does he swallow young birds whole, without enquiring whether they are on the game list or not, but he attacks young lambs and pecks out their eyes, and, as he performs no services which can in the smallest degree compensate for this mischief, he cannot be allowed to exist in a cultivated country. The carrion crow, the Royston crow, the magpie, and the jay have a better account to render: they destroy millions of noxious insects, and if slightly injurious during the hatching season they are eminently serviceable during the rest of the year. The magpie visits the backs of sheep and oxen, and makes a careful search for vermin. The jay is decidedly the least carnivorous of the group, a large part of his diet consisting of peas, beans, and fruit in summer and autumn, and of acorns in winter. These depredations often expose him to death at the hands of gardeners and farmers, but they form no part of Mr. Morant's charges. We should be half inclined to say—"Defend your gardens against the jay by netting, and let him live." Concerning the Royston crow Mr. Morant says—"We must own we once opened the crops of some fully-fledged young hoodies, and found them full of insects, principally beetles. But then their ancestors had eaten eggs for so many years in that country that there were no birds left to lay any within three miles of their nest." We wonder on what evidence this assertion is founded.

The polecat, the stoat, and the weasel come next under examination, and we fear we must approve the sentence

passed upon them by Mr. Morant. These little animals are most determined and bloodthirsty destroyers, not merely of game, but of poultry and of small birds. Waterton says that—"As wrens and robins and hedgesparrows hop from spray to spray, just a few inches from the ground, it seizes them there." He continues—"I once saw a weasel run up an ash-tree, and enter into a hole about 10 feet from the ground. A poor starling had made her nest in it, and as she stood wailing on the branch close by, the invader came out with a half-fledged young one in his mouth, and carried it off." Still Waterton and we believe several naturalists of the present day advocate the preservation of the weasel tribe, on account of the havoc they make among field-mice and rats. "That man only," says Waterton, "who has seen a weasel go into a corn-stack can form a just idea of the horror which its approach causes to the Hanoverians (*i.e.*, rats) collected there for safety and plunder." He winds up his Essay on the weasel by remarking that—"In it may be found the most efficacious barrier that we can oppose to the encroachments and increase of that insatiate and destructive animal, the stranger rat from Hanover." But the remedy, if remedy it be, is very little better than the disease. How is it, further, that the weasel, which a century and a half ago was undoubtedly more numerous than it is in our days, and was less interfered with, still allowed this strange grey rat to become such a formidable interest in the country?

Let us now examine the case of the hedgehog. This unfortunate animal is also tried for his life and convicted. "The hedgehog is the last wild beast on our list,—the 'hypocritical hedgehog,' as Mr. Knox calls him, and 'the most insatiable of all ovivorous British quadrupeds,' whatever his well-meaning and amiable friends may say to the contrary."

"In innumerable instances this little beast has been detected whilst destroying eggs and young birds. Asleep all day, never seen by man unless a dog hunts him out of a hedgerow, he is busy and active enough all night. Can anyone doubt that he is continually finding nests, or do they believe that he ever passes an egg without eating it?

"He has probably no enemy but man, and if man did not reduce his numbers he would do incalculable mischief. Our friends the birds will catch all the insects he is supposed (!) to devour, and we will most certainly do without him as far as possible, hoping to see them [*qy.* the insects?] much more numerous in his place."

In reviewing and reversing this unjust judgment we cannot

help expressing our surprise that Mr. Morant should not have taken the trouble to make himself a little better acquainted with the habits of the unfortunate animal he so sweepingly condemns. We admit that Hoggie is semi-carnivorous, and preys to some extent upon eggs and young birds—when he can catch them. We have known him devour chickens. We have it on good authority that a tame hedgehog once seized a kitten, which was with difficulty rescued by its exasperated mother. We fear that the stories of his occasional raids upon strawberries and windfall apples and pears have some foundation in fact, although no affirmative instance ever came under our own observation. But in spite of all such transgressions the scale must turn in his favour. He is not "supposed," but proved, to devour insects, as was recorded by old Gilbert White, who found fragments of the elytra of beetles among his excreta. His propensity for destroying cockroaches and crickets is known even to the veriest Cockney whose acquaintance with the British fauna was ever gleaned among the bird-fanciers of Whitechapel or of the Seven Dials. The hedgehog's share of work can never be satisfactorily performed by the birds, however they may be multiplied. He gobbles up slugs and snails in the night, their chief time for doing mischief, when the blackbird and thrush are fast asleep, with their heads tucked under their wings, and dreaming perhaps of ripe cherries; he hunts for insects in the bottoms of hedges, and even under ground, where birds are little likely to take up the chase. Hence he is now officially recognised by the French Government as an "agricultural labourer" whose preservation is formally and urgently recommended. But he has still higher claims. Long ago he has been known to be the chief destroyer of the viper, playing in Europe a part similar to that of the secretary-hawk in Africa, and of the mungus in India. Dr. Lenz, a most able and accurate German observer, who studied natural history from a practical point of view, placed the snake-eating habits of the hedgehog beyond all doubt. We have repeatedly witnessed Hoggie tackling a viper, and can testify that the bites of the snake, though they occasionally took effect upon the snout of his enemy, had no more influence than the prick of a needle. The result of the combat was never doubtful, the viper being invariably crunched up with an evident relish. We have known a belt of forest, where we had bagged many a viper, to be cleared of these reptiles by a pair of hedgehogs who had taken up their quarters and reared their brood there. According to a paragraph in "Land and Water,"—

"Vipers abound in the Gironde, and the hedgehog is the declared enemy of the reptile. Since so many hedgehogs have been destroyed vipers have increased at a fearful rate." We are inclined to think that the same cause may have led to the recent multiplication of vipers in England. Now, to form a correct estimate of the services of the hedgehog in keeping down this reptile, we must remember that the bite of the viper is not an insignificant affair. We have known fatal cases in Central and South-Eastern Europe. A young man died last summer from the bite of a viper, received on Leith Hill, Surrey, although medical aid was speedily procured. A French physician, who has published an account of a large number of cases, finds that 20 per cent of the persons bitten succumb to the effects of the venom. Surely, then, a creature which rids us of an evil so serious might claim a better doom than extermination, however many eggs it might devour. We had rather be deprived of pheasants and partridges than be overrun with vipers.

As regards the rat, no naturalist pleads for him. Waterton and Mr. Morant are here perfectly agreed, although they differ as to the means to be employed for his destruction, and although the chapter which the latter allots to the rat is chiefly taken up with a denunciation of "his supposed antidote, the cat." One fact is, however, certain; both rats and mice have of late years enormously increased, and have in some countries become a perfect plague. In Liddesdale the field-mice have devoured even the very roots of the grass. The fields are literally riddled with their holes, and the farmers are in despair. Is it not therefore possible that this increase is due to the destruction, or at least the decrease, of their natural enemies, without any general and organised increase of artificial remedies against their encroachments? The natural enemies of the rat and mouse are the terrier, cat, ferret, polecat, stoat, and weasel; among birds, the larger hawks and the owls; and among British reptiles, the viper. The first of these, the terrier, is, if we are not misinformed, now no longer to be openly and safely used for the destruction of rats, thanks to our humanitarians, who always select some objectionable object for their sympathies—one day rats, another day garotters. It is certainly hard if a farmer, on removing his stack or clearing out his barns, may not send Mustard and Pepper in to seize the grey marauders. The cat is decidedly under-rated by Mr. Morant. We certainly love her not, but so long as mice and rats are common so long Pussy will remain a necessary evil. The increase of field-mice has been most marked where

cats are systematically trapped by game-preservers. Says Mr. Morant—"Does he ever ask himself what these cats go to the woods for? Certainly not to catch rats and mice, which are far more numerous close at home." We doubt all this: leaving field-mice out of the question, we have known many cases where houses much infested with mice were comparatively clear during the harvest season. Mr. Morant himself speaks of the rats venturing out into the hedgerows in the summer months. We maintain that when Pussy lurks among the ripening grain, and springs upon either mouse or rat, or bird engaged in plunder, she is doing mankind good service. The ferret is, of course, a useful agent in the hands of the professional rat-catcher. As for the other animals of the weasel tribe, we have already expressed our mistrust of them. Among birds the great horned owl is a splendid rat-hunter, though we fear Mr. Morant would not approve of his occasional forays upon game. The viper will kill both mice and rats, but we cannot recommend him for toleration even on that account. There are few better rat-hunters in the world than the death-snakes of all countries. Singularly enough the mice and rats, under certain circumstances, retaliate in kind: they eat the eggs of serpents; they attack the young brood, and the adults also when torpid from the winter's cold. The best methods of dealing with rats are already known, and merely require putting in force. The abolition of scamped work about the foundations of houses, the plentiful use of gas-tar or asphaltic preparations in such places, the construction of ovoid drains and sewers where the vermin may find no clinging-room, glass or stoneware pillars for the support of corn-stacks, will greatly limit its sphere of mischief. Poison for rats may best be made up in the shape of candles. The marauders drag these into their holes without suspicion, whilst there is no fear of their being inadvertently eaten by children, or even, we think, by dogs.

So much for the game-destroyers, actual or suspected. Mr. Morant has not a word to say against that loveliest of our native British birds, the kingfisher, nor against the waders and the water-fowl. He praises Waterton as having been in advance of his age in the protection he afforded to these interesting but persecuted creatures, and recommends the owners of parks and manorial domains to follow his example. But what will the anglers say? May they not take exception to a fish-catching bird on the same grounds, and with as much right, as the sportsman brings forward in his protest against hawks and ravens? The diet of the

kingfisher is known beyond the shadow of a doubt. The waders watch on the bank of a pond or in the shallows of a stream till they find opportunity to spear some passing fish: they prey, indeed, also on lizards, frogs, snakes, worms, and insects. But if the angler takes the same view of the case as does Mr. Morant with respect to the carrion crow or the magpie, he will demand their condemnation. Even certain water-fowl, such as the swan, are accused of devouring the spawn of fishes deposited at the bottom of rivers and ponds. We thus find ourselves on the threshold of a serious difficulty. Three conflicting claims are urged. Mr. Morant and the game-preservers bid us destroy the birds of prey and the crow tribe, and preserve all other winged creatures. The anglers, on the contrary, demand the extirpation of the waders and the kingfisher, whatever is done with the Raptores and the small birds. Lastly, but certainly not least, the farmer and gardener pronounce sentence of death on all devourers of seeds and fruits. Here, then, is a "very pretty quarrel." None of the contending powers is willing to abate its own pretensions, though each recommends concession to the other two. The naturalist, if appealed to, stands aghast, and doubts whether he can save any of the feathered race. We are thus led up to a further question, upon which an absolute decision would be premature. We have so far provisionally assumed that the small birds are our benefactors in an unqualified sense, and that their unlimited increase would be desirable; but this is not proven. Few of them are purely insectivorous, as every gardener knows; but, on the other hand, many of the seed-eaters are highly useful by limiting the propagation of weeds. Thus the goldfinch, now becoming rare in many parts of England, is particularly fond of thistle-seed. It is generally said that if we encourage the birds we may avail ourselves of their services, and yet prevent them from doing mischief by the use of netting. We may, indeed, protect wall and espalier fruit in this manner, but to net over entire orchards and fields is impracticable; besides, nets—if they prevent fruit-stealing—will likewise interfere with insect-catching. Further, the alleged depredations of some birds are not deferred until the crops are ripe. The bullfinch, chaffinch, and titmouse are charged with pulling off the buds of fruit-trees. A sentinel with a gun will assuredly scare away the innocent birds as well as those really guilty. One small bird Mr. Morant declines to protect. The sparrow may almost be called a winged rat, from the extent and variety of his depredations; he catches insects during the

breeding season only, and all the rest of the year he is continually in mischief. The evils which have in some instances seemed to follow his local extirpation are probably due, as Mr. Morant very judiciously suggests, to the simultaneous destruction of other birds. Perhaps his worst attribute is that he drives away birds which are at once more useful and more beautiful than himself. His attacks upon the nests of the swallow are well known, and he sometimes drags the helpless nestlings from their cradle and throws them to the ground. One of the most difficult points in practical ornithology is how to preserve other birds without encouraging the sparrow also. Mr. Morant remarks—"Netting them in winter is quite a legitimate way of destroying them, and if they are shot from a trap next morning they will have a chance to escape." We quote this passage as showing a curious idiosyncracy of the mind of the sportsman. Either the sparrow deserves to die or he does not. If he deserves death, why give him a "chance" of life? If he does not deserve it, why seek to kill him? We hold that whatever is worth doing at all is worth doing with absolute certainty, and that "chance" wherever possible should be altogether eliminated.

We see, in fine, that though a "balance of Nature" exists it is incompatible with the presence of civilised man, who is unable to avoid disturbing it even if anxious for its preservation. Hence, how much soever we may approve of the practice of Waterton and of his disciples, in a number of cases we cannot accept them as safe guides. Their general principle, indeed, as Mr. Morant argues, is fundamentally vitiated by the fact that Waterton carried on his observations in a country where all the more formidable beasts of prey have been long ago extirpated. If it be wrong now to root out the polecat, why was it right in former centuries to exterminate the wolf, the bear, and the lynx? To reduce *ad absurdum* the system of letting animated nature alone, we need only try its operations in Assam, Java, the Cape, or any other country still blessed with man-devouring cats and with thanatophidia. Regarding therefore this original balance as provisional, and suitable merely to the absence of man, we have to establish a new equilibrium. But here we have quite as decidedly to reject Mr. Morant's fundamental principle, which is evidently no safe guide. Still more strongly must we protest against the conduct of those who, without enquiry, kill every creature they suspect of mischief.

But having thus expressed our dissent from the views of

others, we must expect to be asked what method we recommend for finding and establishing a new and satisfactory balance? To this we cannot reply in a neat prescription of some half-dozen lines. Much of the knowledge necessary for a decided and final answer has yet to be obtained. We can merely recommend that every case be for the present decided upon its own merits; that the habits, and especially the diet, of all creatures be much more carefully studied than has yet been attempted; and that sentence of extirpation be not passed till the injury done by the species concerned has been proved to be greater than the benefits it confers. But where such proof has been furnished, then let there be no trifling. "*Frappez vite et frappez fort.*" Let there be no "chance" given to the offending animal; let no young ones be spared that some one may exercise his courage and his skill by shooting them down when mature! A few general considerations may assist. There must be, of course, no mercy shown to parasites, internal or external. To say that these creatures "are designed to make people clean" is, as a contemporary remarks, "simply absurd." The cleaner people are the more fiercely they are attacked by bugs and fleas. We once passed the night in a *salasche* high up in the Carpathians, and were nearly devoured, whilst the filthy natives slept on unmolested. War must next be waged against all large Carnivora, and against all or most small Herbivora and Omnivora. It will be useful to take into consideration each separate duty or function which we require animals to perform; to examine what species executes it in the most effectual manner and with fewest drawbacks, and to give that species the preference. Thus the fox, the mole, the hedgehog, the weasel, and the crow tribe, will all destroy cockchafers. But the fox and the weasel—and, in Mr. Morant's opinion, the crows also—have so many vices that man may refuse to employ them as cockchafer-devourers, and may hand over the work to the mole and the hedgehog. Again, the hen harrier and the pheasant both prey upon wireworms; still the pheasant is the safer workman in this department, and we should accordingly give him the preference.

But the greatest difficulty lies in the fact that our animal allies do not spare each other. Thus several beasts and birds have been praised as beetle-destroyers. But among beetles there are multitudes inoffensive; multitudes—as we have already shown—positively useful to man. But the weasel, the crow, and the mole devour the burying-beetle, the dung-beetle, the jardinier (*Carabus auratus*), just as

greedily as the cockchafer and the mole-cricket. The weasel will assassinate his fellow insect-hunter, the mole, just as readily as the rat or the field-mouse. Many naturalists would pronounce the stork useful; but no small part of his time and attention are devoted to catching and swallowing the frog—one of our best friends. The hedgehog kills vipers and noxious insects, and also beneficial ones. The small birds capture the destructive cabbage butterfly, and pick its caterpillars from our fields and gardens, and in so doing they merit well; but they quite as eagerly destroy the "red admiral," the "painted lady," and the "peacock," which help to keep down noxious weeds. Our feathered friends also, when opportunity offers, make a meal of the dragonfly, who in his adult state is zealous as the swallow in clearing the air of winged vermin, and in his earlier and aquatic condition is no less useful in devouring the larvæ of the gnat. All carnivorous insects which inhabit the water merit especial protection. The ladybird helps to rid our fruit-trees, our hop-gardens, and our rosaries of the loathsome aphis, but whilst engaged in this good work it is devoured by the very birds which we are advised to spare and shelter. The ichneumons and other parasitic insects are praised for keeping down caterpillars; yet here again harmless and useful species are attacked quite as frequently as those which are noxious.

Thus our animal allies, like riotous and ill-disciplined troops, exchange blows with each other when they ought to present a front to the common enemy.

Another point remains: we talk of carnivorous and herbivorous species, but in multitudes of cases the diet of animals is by no means so rigidly defined as is commonly supposed: they have their preferences, but rather than starve they are generally ready to adopt a substitute. Cows in Norway are known to partake of herrings in the winter, and in milder climates they are decidedly fond of mumbling a bone. What creature is more decidedly predatory than a spider? Yet when "sugaring" for moths, at night, we have more than once seen a grim spider sitting at the edge of the mixture and apparently sucking it up. We have also met with a *Carabus*, one of the most carnivorous of beetles, similarly engaged, and profiting by the hint we fed one of the same species, in captivity, upon bits of apple. From time to time we hear of a bloodthirsty freak on the part of some beast or bird generally considered a pure vegetarian. Indeed how many orders, or even families, can we find which do not count among their members

both flesh-eaters and plant-eaters, and which may thus be said to dwell on the debatable line? Hence we must consider it as not impossible that if some insectivorous bird—say the starling—should increase to a very great extent, and should become pressed for subsistence, it might perhaps be tempted to attack fruits and grain.

II. ON UNDERGROUND TEMPERATURE, WITH A DISCUSSION OF THE OBSERVATIONS MADE AT SPERENBERG, NEAR BERLIN.

By O. Fisher, Clk. M.A., F.G.S.

THERE is no fact more firmly established in terrestrial physics than that the temperature of the rocks of the earth's surface increases with increasing depth. Observations upon this subject have been made in all parts of the world, and the same result has been everywhere arrived at. Even in the frozen soil of Yakoutzk, in Siberia, this increase of temperature is found to prevail, although the ground is congealed to the whole depth penetrated.* In all mining operations this gradual increase of temperature becomes a very serious consideration, rendering human labour at great depths a very severe trial to the constitution of the workman. And indeed it is this circumstance, more than any other, which fixes a practical limit to the depth at which mining operations are possible. It is not the raising the minerals from profound depths, for the resources of modern engineering are quite competent to overcome any difficulty on that score; it is not keeping the mines clear of water, for they are less troubled with its influx at great than at moderate depths; but it is the impossibility of furnishing the men with an atmosphere to breathe in below the temperature of the blood. For when the air has to be conveyed to long distances it acquires the temperature of the rocks,

* The increase of temperature at Yakoutzk, although the soil is frozen even beyond the depth reached, proves that this freezing is owing to the present low mean temperature of the locality, and that it is not a residual effect of a former glacial and still colder period; for if that were so the strata would be now warmer above than below, whereas the reverse is in fact the case.

and no means have been yet suggested which could furnish it at the atmospheric temperature unaffected—or but slightly so—by its long journey through the subterranean passages.

This increase of temperature, though universal, is not everywhere the same. The average is about 1 degree Fahr. for between 50 and 60 feet of descent. Such is the result of very numerous observations. Some of these have been made by drilling holes in the rock in deep mines; others by lowering thermometers in Artesian bore-holes. A Committee was appointed by the British Association to report upon the subject, and their reports extend from the year 1869. Much information upon the matter may also be gathered from the Reports of the Parliamentary Commission upon the Coal Supply.

Unquestioned as is the fact, nevertheless the cause of this increase of heat has formed the ground for much speculation. The most obvious explanation of it is offered by the phenomena of hot springs and volcanos. These show us that, in some places at any rate, much higher temperatures exist at great depths than have ever been reached by artificial perforations. But these phenomena might be said to be local, while the general slow increase of temperature we have been describing exists more or less at every place. Are these phenomena directly connected? Are they indirectly connected? Or, are they altogether unconnected? Various answers have been returned to these questions.

Among "practical men" engaged in mining operations an opinion seems to have prevailed that the increase of temperature in deep mines is due to the pressure of the overlying strata or "cover." We may unhesitatingly dismiss this hypothesis. Pressure by itself cannot develop heat. Where motion is destroyed as motion, there it is that it is converted into heat. Thus the motion which is destroyed when a hammer strikes upon an anvil will develop heat that can explode fulminating powder, or heat a nail red-hot. So, also, the bearings of a wheel become heated by friction, which gradually destroys its motion. But though mountains rise on mountains, there will be no heat produced unless motion of some kind is destroyed. In deep coal-mines an effect called "creep" is caused by the enormous pressure upon the sides of a passage, or upon the pillars left to support the roof, causing the floor of the excavation to swell up. In this case there is immense friction between the particles of the rock, were it not for which the passage would become instantaneously closed. It has been remarked that considerable heat sometimes accompanies this creep,

in which, be it observed, we have not pressure alone, but motion destroyed and converted into heat.

Sir Humphry Davy ascribed the volcanic fires to the oxidation of earthy and metallic bases, supposed by him to exist low down in the interior of the earth. Since water is everywhere present in the crust, if it be absorbed by the abstraction of its oxygen when it encounters the bases, and by the blowing off from volcanic vents of the equivalent hydrogen, then its place must be continuously supplied by the percolation downwards of fresh accessions; and thus a generally diffused increase of temperature was supposed to arise, and to manifest itself by its escape towards the surface.

A very fascinating theory, which has met with support among many scientific men, has of late years been promulgated by Mr. Mallet, who considers the heat of volcanic action to be derived from the transformed work of crushing the rocks of the earth's crust, owing to the contraction of its interior through long-sustained cooling. The cause of evolution of the volcanic heat under this theory is of the same kind as that alluded to above, in the case of creeps; while the ubiquitous increase of temperature in descending into the earth is looked upon as chiefly a manifestation of the generally diffused heat of the interior, by the escape of which the contraction in volume is produced. Volcanic action, Mr. Mallet tells us, is caused by this contraction manifesting itself locally and paroxysmally.

In order to decide what theory best accounts for the phenomena, it is obvious that the first step is to make sure of the facts themselves. In other words—What is the law regulating this increase of heat? At what rate does the temperature augment? This might be supposed to be a point easily settled. It might be supposed that nothing would be easier than to insert thermometers into the rock at different depths in a mine, and to read off their indications; or to lower them into a bore-hole for the same purpose. But there are many difficulties to be overcome, and a host of disturbing causes present. The rock, or the face of the coal in a mine, is affected by the temperature of the air. The air is warmed by the presence of men and horses; it is cooled by the ventilation carefully kept up. Hence the surface of the working is not at the true temperature of the rock. The result of careful experiments made by Sir G. Elliot showed that when thermometers were inserted in the coal in a long-wall working, at distances of 3, 6, and 12 feet from the face, no alteration could be

detected in the indications given beyond 3 feet from the face of the coal; and his observations were accordingly taken at 4 feet. It is evident, as will be seen by what will be said hereafter, that the necessary distance must be governed by the difference between the temperature of the ventilating air and of the seam, and that the greater this difference the farther it would be necessary to bore into the coal to obtain an estimate of its true temperature. If it be attempted to ascertain the law of increase by means of an Artesian bore-hole, there are here also disturbing causes present. The action of the tool warms the rock by mechanical means, the friction or pounding action developing considerable heat. Hence while the work is in progress no reliable observations can be taken, except during intervals of suspension, and then it appears—as will be seen further on—that the interval needs to consist of weeks rather than of days. When the work is completed, and the bore stands nearly full of water, it is the temperature of the water which is obtained on lowering a thermometer into it, and we cannot be sure that that coincides with the temperature of the rock. In fact many reasons may be assigned why it should not do so. Springs may enter on one side and flow out at the other; or they may rise from lower levels and flow away at higher. The very act of lowering the thermometer tends to mix up the differently heated layers of water, and to confuse the result.

A single instance will suffice to illustrate the irregularity of increase referred to. At Rose Bridge Colliery the temperatures were taken by drilling a hole a yard deep at the bottom of the shaft during the process of sinking. If the mean temperature of the surface there be taken at 50° F., the mean rate of increase for the whole depth was 1 degree for 55 feet. But at the successive depths of 605, 630, 663, 671, 679, 734, 745, 761, 775, 783, 800, 806, 815 yards respectively the rate appears to have been 1 degree for 70, 25, 49, 24, 110, 66, 32, 48, 51, 36, 54 feet.

Within the last few years a very deep boring has been carried down at Sperenberg, near Berlin. This has reached the extraordinary depth of 4052 Rhenish feet, or 4172 British feet. A most surprising geological fact about this boring is that, with the exception of the first 283 feet, which were carried through gypsum with some anhydrite, the remainder passed entirely through rock salt. This seemed to offer an exceptionally good opportunity for observing temperatures in a homogeneous rock, which might be expected to be comparatively free from the sources of error arising from

variable heat-conducting power in the layers successively penetrated. The observations in the bore were taken with much precaution, under the direction of Herr Eduard Dunker, Inspector of Mines. A *resumé* of his paper upon them will be found in "Nature" (No. 376, for January 11 of the present year) as contained in the "Ninth Report of the British Association Committee on Underground Temperature." In No. 312 of the same publication (1875) there had previously appeared a notice of a contribution, by Prof. Mohr, of Bonn, to the "Neues Jahrbuch für Mineralogie" (1875), in which that gentleman had commented upon the law of increase which he supposed to be deducible from the observations made in this bore-hole. The result at which he arrived was remarkable enough, and if it could not have been explained away would have justified the conclusion which he drew from it, which was that the source of heat within the crust of the earth was situated within the crust itself, for that after a certain depth was reached—which he put at 5170 feet—the increase would be *nil*. However, in the Report of the British Association just published, it is explained how this result had been arrived at, and it is distinctly shown that Prof. Mohr's conclusion was in reality not based upon the original observations themselves, but that it arose from the form of an empirical mathematical formula which Dunker had assumed to express the law; so that the law which brought the increase of heat to *nil* at 5170 feet was of Dunker's making, and not Dame Nature's. In fact it had been implicitly assumed that the increase would come to an end, and all that Mohr did was to find out at what depth it would do so, supposing that assumption true. A better instance can hardly be required to show the extreme caution necessary in accepting the conclusions of philosophers when they are, on the face of them, heterodox. The safe attitude of the mind in such a case is to suspect that there must be some mistake, and the duty of the competent is to try and find it out.

This bore-hole, from the favourable circumstances already referred to, and the care with which the observations were made, deserves full consideration. The temperatures were taken in two manners. In one set of observations the temperature of the water in the bore-hole was observed with a suitable thermometer; in the other an apparatus called a geo-thermometer was lowered, which cut off a portion of water from that above and below it by two disks or bags. A thermometer was enclosed in the space between the disks, and, after the thermometer had been down not less

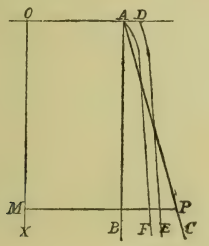
than ten hours, the temperature shown by it was assumed to be that of the rock at the corresponding depth. This was found to differ sometimes by more than a degree Réaumur from the temperature of the water at the same depth, as shown by the first-named thermometer, when the water above and below was not cut off. Now, it can hardly be supposed that so great a difference as this really existed simultaneously between the surface of the rock exposed to the water in the bore-hole and the water itself. On the contrary, it seems evident that the surface of the rock, being exposed to the action of the water, must have assumed the temperature of the water, whatever that might be, at any given depth. But the water was undoubtedly affected by convection currents, and consequently its temperature was not the same as that of the rock *in mass* at a distance from the bore-hole. Hence, when the circulation of these currents was stopped by the disks, the surface of the bore-hole would begin to tend towards the true rock temperature. If the water at any depth was warmer than the rock in mass, its temperature would begin to fall when the currents were cut off, and if the water was cooler than the rock it would begin to rise.

Let us, then, shortly consider the general effect of convective currents as they would affect the water.

These currents arise from the circumstance that when water is warm it expands, and consequently becomes lighter. This expansion is exceedingly small; yet in a substance of such extreme mobility as water it is sufficient to cause the expanded portions to rise, while the denser portions above sink to supply their place. The expanded portions in rising carry their heat up with them, and the cooler portions in sinking reduce the temperature of the lower part of the column. If therefore we had a column of water originally warmed towards its lower portion, but not supplied there with fresh accessions of heat, the whole would, if open to the atmosphere, shortly assume an equable temperature throughout.

Let us now invoke the aid of a diagram to render our ideas more clear; and suppose the depths in the bore-hole to be measured along the vertical line O X, and let lines drawn at right angles to this represent, in proportion to their lengths, the temperatures at the corresponding depths. Let O A represent the mean temperature of the surface of the ground. Then on the supposition that the temperature of the rock in mass increases proportionally to the increase of depth, the temperature of the rock will be represented by a *straight* line, as A C. If, then, the temperature of the

water in the bore-hole correctly gave the temperature of the rock, its temperature would be likewise represented by the same straight line A C. But since it is affected by convective currents this cannot be the case : for they tend to warm the upper portions of the column of water, and to cool the lower. Hence, so far as we have gone, the water in the upper part of the bore-hole will be warmer than the body of the rock, and in the lower part it will be cooler ; and in our diagram we must draw a line to represent its temperature, more distant from O X in the upper part, and nearer to it in the lower. It need not, however, be a straight line, the law of increase of temperature being possibly altered. Suppose, then, D E to be this line, or the curve of temperature of the water on the supposition now made. It will be seen that it must intersect the line A C.



There is a further consideration to be taken into account. If the bore-hole is nearly full of water, and of considerable dimensions, it will present a considerable surface to the atmosphere. At Sperenberg the water stood 7 feet from the stage of the bore-pit, and the bore-hole was a foot in diameter. The consequence would be that the open surface of the water would be cooled, sensibly to the temperature of the air. This would bring the curve of temperature of the water at the surface nearly to the same point as the temperature line of the rock there, and it would also have the effect of reducing the temperature of the water throughout the column, below what it would be if it had not this extrinsic cause of cooling. The ultimate result would be that the temperature curve of the water would assume some such form and position as A F. It will be observed that this line also intersects A C ; and the signification of this is, that the water in the bore-hole is warmer than the rock mass in its upper portion, and cooler than the rock mass in its lower portion ; or, in other words, the water tends to warm the rock in the upper portion, and to cool it in its lower portion. Consequently, if the circulation of the currents be interrupted, the water im-

prisoned between the disks of the geo-thermometer would in the upper portion gradually become cooler, as heat was conducted away from it into the body of the rock; and it would gradually become warmer in the lower portion, as heat was conducted into it from the body of the rock. But at the particular depth at which the two lines A C and A F intersect no alteration would take place.

That such an effect was actually produced is shown by the following table, the first two columns of which are taken from Dunker's Table II., in his paper entitled "Ueber die Benutzung tiefer Bohrlöcher zur Ermittelung der Temperatur des Erdkörpers, und die desshalb in dem Borloche I zu Sperenberg auf Steinsalz angestellten Beobachtungen." The two temperatures marked with asterisks differ from those quoted in the British Association Report from Dunker's quarto paper of 1872 in the "Zeitschrift für Berg-Hütten und Salinen-Wisen." But they are printed as here given in the octavo paper from which they have been copied, and are stated there to be the mean results of several observations. Dunker considered the deepest observation with the geo-thermometer unsatisfactory.

Depth in feet.	Temp. R. Water shut off.	Temp. R. Water not shut off.	Water shut off, consequent alteration of Temp.	Surface Temp. 7·18°. Absolute Increase at each depth in Col. 2.	Being at the rate of 1° R. per No. of feet.	Absolute Increase at depths.	Being at the rate of	
							1° R. per ft.	1° F. per ft.
15	9·40	10·35	−0·95	—	—	—	—	—
30	9·56	10·20	−0·64	—	—	—	—	—
50	9·86	10·40	−0·54	—	—	—	—	—
100	10·16	12·30	−2·14	2·98	33	—	—	—
300	14·60	13·52	+1·08	4·44	45	—	—	—
400	14·80	14·30	+0·50	0·20	500	—	—	—
500	15·16	14·68	+0·48	0·36	277	—	—	—
700	17·06	16·08	+0·98	1·90	105	—	—	—
900	18·50	17·18	+1·32	0·44	455	—	—	—
1100	*19·90	19·08	+0·82	1·40	143	11·72	94	42
1300	21·10	20·38	+0·72	1·20	166	—	—	—
1500	22·80	22·08	+0·72	1·70	118	—	—	—
1700	24·10	22·90	+1·20	1·30	154	—	—	—
1900	25·90	24·80	+1·10	1·80	111	—	—	—
2100	*27·70	26·80	+0·90	1·80	111	7·80	128	57
2300	28·50	28·10	+0·40	0·80	250	—	—	—
2500	29·70	29·50	+0·20	1·20	166	—	—	—
2700	30·50	30·30	+0·20	0·80	250	2·80	214	95
2900	—	31·60	—	—	—	—	—	—
3100	—	32·70	—	—	—	—	—	—
3300	—	33·60	—	—	—	—	—	
3390	36·15	34·10	+2·05	5·65	122	—	—	—
3500	—	34·70	—	—	—	—	—	—
3700	—	35·80	—	—	—	—	—	—
3900	—	36·60	—	—	—	—	—	—
4042	38·25	38·10	+0·15	2·10	310	7·75	212	95

In the above table the first column gives the depth in Prussian feet, at which the observations of temperature were made.

The second column gives the temperatures observed at the respective depths when the convection currents were stopped by the use of the geo-thermometer.

The third column gives the temperature of the water in the bore-hole when the convection currents were allowed to circulate.

The fourth column gives the alteration of temperature at each depth which arose from stopping the convection currents—*minus* when the temperature fell, *plus* when it rose.

The fifth column gives the increase of temperature at each depth over that at the previous one, as taken from the second column.

The sixth column gives the rate of increase at each depth, measured by the number of feet which it would be required to descend to gain an increase of 1 degree Réaumur, on the supposition that the increase per foot remained constant throughout that depth; consequently large numbers show a proportionately slow increase.

The seventh column, like the fifth, shows the increase of temperature at the depth against which the number stands, over that at the depth at which the previous number stands; the object of this column being to obtain averages at longer distances apart.

The eighth column, like the sixth, shows the rate of increase of temperature with longer averages, measured by the number of feet of descent for 1° Réaumur.

The ninth gives the same increase when measured in feet of descent for 1° F.

The difference between a Prussian and an English foot is so small that, for the purpose in hand, they may be considered equal.

In discussing this table the first point to be noticed is that the rate of increase is by no means so equable as, from the homogeneity of rock, it might have been expected to have been. In order to obtain anything like a general law of increase, it is necessary to take the average of the increase at considerable distances apart.

The second point has been already adverted to, viz., that the shutting off of the convection currents caused an increase of temperature in the upper part of the bore-hole, and a diminution of it in the lower. This shows clearly that the temperature of the rock was altered temporarily, by the action of the convection currents, to some distance away laterally from the bore-hole. The hole was lined with three tubings, one behind another, for the upper 440 feet, and, on account of the pre-

sumed convection currents still existing behind the tubing, the observations in the upper part of the column were not considered trustworthy. But this circumstance does not militate against the conclusion at which we have arrived, because whatever effect convective currents might have upon the temperature would be *pro tanto* produced by these concealed currents, so that all they could do would be to lessen the effect of shutting off the currents in the main channel. And we may reasonably suppose that the change of temperature arising from that proceeding would have been still greater than recorded, if the tubing had been in close contact with the rock.

In the discussions of these observations hitherto published* the observations made with the geo-thermometer have been looked upon as giving a near approximation to the temperature of the earth's crust at this locality; and, consequently, it is these which we propose to consider further. Although the sixth column shows that the rate of increase of temperature was far from equable when comparatively short intervals are taken into account, yet, when we pass to the eighth and ninth columns, we observe that on the whole there is a decided diminution of the rate of increase in the lower depths. It was probably this circumstance which induced M. Dunker to assume that empirical formula for the law of increase which led Prof. Mohr to believe that, at the depth of 5170 feet, the increase would be *nil*, and thence to conclude that the source of the heat of the crust must be situated within the crust itself, instead of—as is usually supposed—coming up from the profound depths below. The question which presents itself therefore (and it is a most important question) is—Can this diminution of the rate of increase in the indications of the geo-thermometer be consistent with an equable rate of increase in descending as deep as the observations went into the earth's crust? The answer, that it can, seems to follow from the considerations already made on the effect of convection currents upon the temperature, not of the water only, but of the rock itself. It is obvious that the continued contact of water at a different temperature from that of the rock must alter the temperature of the rock itself where it is in contact with the water. And it has been remarked that, almost beyond dispute, it had actually that effect; consequently the rock in contact with the water must have been cooled in the lower part of the bore-hole. Moreover, the principal cooling

* Since this article was sent in, the writer has met with a course of lectures entitled "Vorträge über Geologie, von F. Henrich, Wiesbaden, 1877," in which the Sperenberg observations are discussed with much acumen.

effect of the currents would occur towards the bottom of the hole, where the cold water coming down from above would tend to accumulate, because there would be no warm currents coming up from below to disturb it.

When, therefore, we enquire how far the observations made with the geo-thermometer can be depended upon as having given the true temperature of the rock in mass, two questions present themselves. First, could the water enclosed between the disks assume eventually the temperature of the rock mass, if the instrument were left down long enough? And, secondly, if it could do so, was it in fact left down long enough?

Before attempting to answer these questions, it may be well to refresh our minds regarding the mode by which a material like rock transmits heat through its substance, and by so doing affects the temperature of whatever may happen to be in contact with it. In such a case the heat is not conveyed from point to point by the fetch-and-carry system which, going on in a liquid like water, is called convection, and has been already described; but each particle of rock receives heat from those in contact with it if they happen to be hotter than itself, and gives up heat to those in contact with it which are cooler than itself, without ever moving from its place. Thus the heat is passed on from particle to particle, much as we see buckets of water passed from one person to another at a fire. This mode of propagation of heat is called conduction, while the power inherent in any substance to transmit heat through itself is called its conductivity; those substances which can transmit heat most rapidly being said to have greater conductivity than those which transmit it more slowly. The conductivity of ordinary rock is small; that of rock salt is said to be comparatively great.

Could, then, the water enclosed by the geo-thermometer recover the temperature of the rock mass if the instrument were left down long enough? It appears that this result could be only partially attained, however long it were left in the bore-hole. The convective currents in the water would affect the temperature of the column at the upper and under surfaces of the enclosing disks of the instrument. And even if the disks were purposely made of such badly conducting material as to prevent any temperature effect from the currents being carried through the disks, still such effect would be conducted round the edges of the disks, through the substance of the rock itself; consequently, however long the instrument might have been allowed to remain down, the temperature of the water enclosed be-

tween its disks would have been to some extent affected by the convective currents; and their tendency in the lower parts of the column would be to reduce the temperature, and diminish the rate of increase.

A method of obviating this source of error is mentioned in the British Association Report, already referred to, as having been lately suggested by Sir W. Thomson. It consists in using a series of india-rubber disks placed at considerable distances apart.

Putting the above-discussed source of error aside, we come to our second enquiry, whether the geo-thermometer was left down long enough for the water between its disks to assume the temperature of the rock mass ?* Let us, then, consider the conditions of the system before the geo-thermometer is introduced. We have a very long vertical column of water enclosed in a cylindrical hole within a mass of rock, the rock extending to an infinite distance both sideways and downwards. The water in the bore-hole at any given depth has, in consequence of the currents, a temperature which differs from that of the rock on the same horizon at a distance from the hole. This temperature of the water may be higher or lower than that of the rock; but we will suppose it lower, as it will be in the deeper parts of the hole. The rock surface of the bore-hole, being constantly laved by the water, has been brought to the same temperature as the water. It necessarily follows from this that if we could examine the temperatures of the rock at greater and greater distances from the bore-hole, we should find them become higher and higher, and we should have to penetrate to some considerable distance before the increase came to an end, and when it did so we should feel assured that at last we had reached rock of the true temperature for the depth. The greater the difference between the temperatures of the water and the rock in mass, the further we should have to penetrate for that purpose, and this would be furthest in the lower portions. Now suppose the currents interrupted by lowering the geo-thermometer, so that the heat ceases to be conveyed away from the rock-surface of the bore-hole; and we will now dismiss the consideration of the effect of the water above and below the disks of the instrument. The heat which

* It appears that at the depth of 1100 feet an observation of ten hours duration gave the same result as one of nineteen hours; whence it was concluded that ten was long enough. But considering the great thermal capacity of water, and the small changes which, except just at first, might be expected in the rock, as well as the difficulty of the operation, this trial can hardly be held conclusive against the probability of a further increase of temperature, if the geo-thermometer had remained down longer.

flows out of the sides of the bore-hole into the imprisoned water is now no longer conveyed away by the currents, and begins to accumulate there. The rock in the neighbourhood of this water begins to get warmer. The region of the true rock-temperature approaches nearer and nearer to the bore-hole, until at last it reaches it, and then, and not until then, the imprisoned water assumes the true temperature of the rock. This process will require time, and that a long time. In the first place, on account of the great capacity of water for heat, it requires much more heat to warm up a volume of water through a given range of temperature than it would require to warm up an equal volume of rock. In the second place, it requires a long time for the heat to travel through the rock to reach the water. And although the conductivity of rock salt may be considerably higher than that of ordinary rock, so that the heat passes more quickly through it, yet there will be a compensating effect in the present instance, because, on account of the greater conductivity, the true rock temperature would not be reached within so small a distance from the bore-hole, so that the heat would have a longer distance to travel before the true temperature could be restored. That the geo-thermometer was not left in place sufficiently long for this purpose seems to be proved by the following instance of the extreme slowness with which the passage of heat under such circumstances takes place. From comparing the British Association Reports for 1872 and 1873 we gather that, in the course of the observations made at the Artesian well of La Chapelle, at S. Denis, it was noticed that the temperature at the bottom of the hole was much affected by the action of the "trepan" or boring tool. It appears to have been expected that this effect would have passed off in a few days. Accordingly, a week after the tool was stopped, observations were taken, and there was found what was thought to be an unaccountably sudden increase of nearly 8° F., in 60 feet of descent, near the bottom of the hole. But in the following year a second set of observations were made "some months" after the boring had been suspended, and the abnormal increase was found to have entirely disappeared. We learn, then, that *a week* was not sufficient for the rock wall of the bore-hole to regain its balance of temperature after having been disturbed through about 7° F. How much longer was required we have not the means of knowing. It seems a necessary inference that a few hours would be quite insufficient for a correct observation, and the time allowed at Sperenberg did not exceed such an interval. The conclusion must therefore follow that, on both the accounts referred

to, the geo-thermometer must have shown in the lower depths a temperature less than the true temperature of the rock of the earth's crust existing at that depth.

The reasons given above seem quite sufficient to account for the diminished rate of increase of temperature shown in the table; and they would lead to the conclusion that in all observations made in bore-holes of a sufficient width to allow convective currents full play, the temperatures taken at the lower parts will—even with the geo-thermometer—be less than the true rock-temperature. The conclusion appears a legitimate one that the diminution in the rate of increase in a bore-hole, even when the convection currents are temporarily shut off, does not necessarily imply that there is any such diminution of the rate in the body of the rock; or, in other words, that the temperature curve within the rock may be a straight line, although that given by the geo-thermometer be concave to the axis of depths. It will also follow that the mean rate of increase for the whole depth within the rock will exceed that shown by the geo-thermometer. The mean increase, for instance, in the above table, between the temperature of the surface and that at 4042 feet, is 1° R. for 129 feet, or 1° F. for 57 feet; and it is stated in the Association Report that, when the observations are corrected for pressure, the mean rate to 3390 feet is 1° F. for 51·5 English feet. This is about the usual average. We may therefore conclude that at Sperenberg the actual rate of increase in the body of the rock is greater than this.

The most important conclusion from the above is that, in spite of the decreasing rate shown at Sperenberg (and also at St. Louis, and perhaps other places), the old assumption may be correct, that for such depths as have been reached the rate—if it could be truly observed, and setting aside local causes of disturbance—is probably a uniform rate, amounting to, or, as the above reasoning would lead us to infer, probably exceeding, 1° F. for 51 feet of descent. Are we, then, to conclude that this rate of increase obtains for all depths, however great? If such be the case, as Sir William Thomson in his late Sectional Address at the British Association (1876) sarcastically observed, "by a simple effort of the geological calculus it has been estimated that 1° per 30 metres gives 1000° per 30,000 metres, and 3333° per 100 kilometres." And since it has been considered that between the two last-named temperatures all—even the most refractory—substances of the earth's crust would melt; therefore it has been concluded that at depths varying from 30 to 100 kilometres (or from about 20 to 60 miles) the temperatures are so great as to melt all known substances.

The argument, we are told, has been used by a geologist that, because the increase has been found proportional to the increase of depth as far as observations have gone, "it is most in accordance with inductive science to admit no great deviation in any part of the earth's solid crust" from the law of proportionality that has been observed as far down as observations have extended. The truth seems to be that the uniformitarian principle had led this geologist (whose name is not mentioned) to argue from the known to the unknown according to an unvarying law; and also that he held the opinion that the interior of the earth was fluid. According to this assumption his conclusion might not be far from correct, because the condition requisite for a uniform rate of increase is simply that a fixed temperature should be maintained at a fixed depth. Now, if the interior of the earth were fluid, and affected by convective currents, such would be very approximately true. For a fall of temperature could not take place at the fixed depth without equally taking place throughout the fluid nucleus. And it is obvious that an enormous lapse of time would be necessary to reduce the temperature of so vast a reservoir, by any sensible amount, by means of the small secular loss which takes place through the outer crust. Although, then, the sarcasm is not without point, that geologists as a race employ no higher method in their calculations than the "Rule of Three," nevertheless, upon the hypothesis probably in this geologist's mind, that was indeed the proper rule to use, and the conclusion would have been correct that the central fluid would be found at some depth within the limits named.

Sir W. Thomson's mode of reckoning the rate of increase proceeds upon a different plan. From astronomical and tidal considerations he has satisfied himself that the earth has not a fluid nucleus, and, from his mode of viewing the manner of its consolidation, he believes that it passed from the state of a fluid to that of a solid globe in a comparatively short space of time; since which period all geological events have happened. He has adapted a mathematical formula to express the rate of increase of temperature, at any given depth, in terms of the length of time since this complete consolidation took place; and by means of this formula he performs those seemingly marvellous computations which are so astonishing to the uninitiated. Now, it is a remarkable fact that, for reasonably small values of the time in question,—in other words, of the age of the habitable world,—and for all such depths as the puny efforts of man have been able to reach, it would be impossible to perceive any deviation from a uniform rate of increase. But

if it were possible to reach such depths as the geologist ventured to speak of, the rate, on Sir W. Thomson's hypothesis, would be found to be sensibly diminishing, and, even in the centre of the earth, a greater temperature need not exist than what he expected to find at 60 miles. The greater the value we assign to the presumed age of the world, the further down we should have to dig before we should find the rate sensibly diminishing. Some interesting particulars upon the relation between different assumed values for the world's age, and the depths at which it might be practicable to detect a diminution in the rate of increase of temperature, will be found in the address referred to. But the conclusion at which, upon the whole, we must arrive is that—on the supposition that the heat of the earth's crust comes up from below (and is not generated, as Prof. Mohr supposed, within itself)—no mine nor bore-hole is ever likely to be put down deep enough to solve the question whether the earth is solid from surface to centre, or not so. If ever it could be conclusively proved, by observation, that the rate began to diminish, that would be an argument that the earth is a solid cooling by conduction from its entire mass. But the shortest age which can be assigned to the world's history is too long to allow us to expect that such a depth will ever be reached.

The object of the portion of this article which relates to the observations at Sperenberg has been to demonstrate that the diminution in the rate of increase supposed to exist at that place was only apparent, and could be accounted for without assuming any such diminution of the rate to exist in the rock itself.

As a fact, however, the geologist has to deal only with the surface phenomena of the globe. It is a matter beyond his immediate province to enquire what may be the condition of the earth's interior, except to just such an extent as that must influence the surface conditions. Accordingly he is ready to accept from the physicist such conclusions as can be arrived at solely by methods outside his own province of study. But he asks the physicist, in return, not to ignore the well-ascertained conclusions of geology, nor to suppose that the mistake must certainly be on the geological side if they do not square entirely with his own results. The writer would beg to be allowed to submit that there are some things which do not well agree with Sir W. Thomson's conviction that the earth became solid throughout, and has since cooled as a solid; for such a supposition will not account for the phenomena of surface inequalities, as geologists have almost unanimously agreed to interpret them.

It is almost universally believed that the inequalities of the earth's surface were produced, and are maintained against degradation, by the contraction of its volume. The writer some time ago made a calculation* of the magnitude which these inequalities might be expected to have assumed, on Sir W. Thomson's view, expecting fully that a cooling solid earth would account for them all, and for losses by denudation besides; but to his surprise they came out far smaller than those actually existing. The elevated tracts and the ocean bottoms present deviations from a spherical surface much greater than could arise from a cooling solid sphere. Capt. Dutton, an American geologist and physicist, was led, by a similar consideration, to abandon the contraction theory altogether, and to seek for some other explanation of the formation of the inequalities. In short, it seems clear that if Sir William Thomson's views upon this point are correct, then the inequalities of the earth's surface are not due to the contraction of the sphere, and the plications of the strata must be accounted for in some inexplicable way. If, on the other hand, the inequalities of the surface are due to contraction, there is strong reason to doubt that the earth became solid throughout, and that it is now cooling as a solid, according to Sir W. Thomson's law.

There is another fact connected with mountain chains which has an important bearing upon this question. To adopt Prof. Green's description of these :—"They owe their superior elevation to the fact that the rocks of which they are composed have been squeezed and ridged up to a greater height than the rocks of the country on either side."† Now this peculiarity, as was pointed out by Capt. Dutton, and also implied by the writer‡ in his argument against Mr. Mallet's theory of volcanic energy, necessitates a "slip" of the earth's superficial strata over whatever it be that underlies them. It is obvious that a more or less perfectly fluid substratum is requisite to account for this peculiar character of mountain-chains. What this fluid condition may be, whether permanent or transient, whether it is connected with volcanic action or not, are questions open to discussion. But there must be—if not permanently, at least during epochs of the mountains rising—some such condition present. Indeed the nearest analogy to the formation of a mountain-chain seems to be found in the crushing together of the adjacent edges of two sheets of floating ice, the flotation of the sheets being a necessary part of the

* Cambridge Phil. Transactions, 1873.
† Geology for Students and General Readers, p. 462.
‡ Phil. Mag., October, 1875, p. 9.

conditions. It was from noticing the ridges thus formed in ice that the writer originally took his ideas upon this class of geological questions.

The writer's own theory, which he is proud to say was approved by the late Mr. Scrope very shortly before his death, is, that there exists such a stratum in a state of igneo-aqueous fusion, and that it is to the steam escaping from this, and so diminishing its volume, that the contraction of the globe and the consequent compression of the crust are mainly due. He thinks it probable that at least a large portion of the water now composing the oceans has by this means been transferred from the under to the upper side of the solid crust. How thick this lubricating layer may be he does not undertake to say, nor how deep down. If, however, such does exist, there will be probably some change in the physical relation of the substance of the earth as regards heat, at the surface of this stratum, such as would render the laws of the simple conduction of heat out of a hot solid globe towards the cold of space inapplicable to found upon them any theory as to the age of the world, or of the distribution of heat in its interior, based upon the observed rate of increase of temperature in the crust.

III. MOVEMENTS OF JUPITER'S CLOUD-MASSES.

By Richard A. Proctor.

IF Jupiter be regarded as a planet resembling our earth in condition, we find ourselves compelled to believe that processes of a most remarkable character are taking place on that remote world. It is singular with what complacency the believers in the theory that all the planets are very much alike accept the most startling evidence respecting disturbances to which some among those brother worlds of ours must needs on that hypothesis have been subjected. Mighty masses of cloud, such as would suffice to enwrap the entire globe on which we live, form over large regions of Jupiter or Saturn, change rapidly in shape, and vanish, in the course of a few minutes; and many are content to believe that what has thus taken place resembles the formation, motion, and dissipation of our own small clouds, though the sun pours but about a twenty-seventh part of the heat on

Jupiter, and but about a hundredth part on Saturn, which we receive from his rays. The outline of Jupiter, as indicated by the apparent position of a satellite close to his disc, expands and contracts through thousands of miles, yet the theory that Jupiter is still intensely hot must not for a moment be entertained, though the expansion and contraction of the solid crust of a cool planet through so enormous a range would vapourise a portion of its mass exceeding many times the entire volume of our earth. Saturn is seen by Sir W. Herschel and Sir J. Herschel, by Sir G. Airy, Coolidge, the Bonds, and a host of other observers, to assume from time to time the square-shouldered aspect, a change which—to be discernible from our distant standpoint—would imply the expansion and contraction of whole zones of Saturn's surface through 4000 or 5000 miles at least; yet it is better to believe that these stupendous changes have affected the solid crust of a planet like our earth, than to admit the possibility that the outline we measure is not that of the planet itself, but of layers of cloud raised to a vast height in the deep atmosphere surrounding a planet still glowing with its primeval fires.

The phenomena I am now about to consider belong to the same category. They are utterly inexplicable, or only explicable by the most sensational assumptions as to the processes taking place on Jupiter, if we adopt the old theory of Jupiter's condition; while if we regard Jupiter as an intensely-heated planet surrounded by and entirely concealed within a cloud-laden atmosphere several thousand miles in depth, they at once admit of the most simple and natural explanation.

It has, of course, long been known that the belts of Jupiter are phenomena of his atmosphere, not of his surface. The belts of lightest tint have been regarded as belts of cloud, and the darker belts as either the real surface of the planet seen between the cloud-belts, or else as lower cloud-layers, appearing darker because in shadow. Accordingly, when features of the belts have been watched in their rotational circuit, it has been clearly recognised that the rotation determined in this way is not necessarily or probably the true rotation of the planet itself. Further, it has been proved, beyond all possibility of question, that some at least among the spots upon the planet's belts have a motion of their own; for whenever two spots in different Jovian latitudes have been observed, it has been almost constantly noticed that the one nearer the Equator has had a greater rotation rate than the other. Again, it has sometimes

happened that instead of two spots, in different latitudes, a well-defined dark streak or opening, having its two extremities in different latitudes, has remained long enough to be observed during several rotations of the planet. In these cases it has been observed that the end of the streak nearest the Equator has travelled fastest, not only absolutely, but in longitude, insomuch that the position of the streak has notably altered.

Thus, in February, 1860, Mr. Long, of Manchester, noticed across a bright belt an oblique dark streak. "Its position" (I quote from a paper of my own written six years ago, when as yet the theory now before us was in its infancy) "might be compared to that of the Red Sea on the globe of the earth, for it ran neither north and south nor east and west, but rather nearer the former than the latter direction. The length of this dark space—of this rift that is, in the great cloud-belt—was about ten thousand miles, and its width at least five hundred miles; so that its superficial extent was much greater than the whole area of Europe." It remained as a rift certainly until April 10th, or for six weeks, and probably much longer. It passed away to the dark side of Jupiter, to return again after the Jovian night to the illuminated hemisphere, during at least a hundred Jovian days; and assuredly nothing in the behaviour of terrestrial clouds affords any analogue to this remarkable fact. "This great rift *grew*, lengthening out until it stretched across the whole face of the planet, and it grew in a very strange way; for its two ends remained at unchanged distances from the planet's equator, but the one nearest to the equator travelled forwards (speaking with reference to the way in which the planet turns on its axis), the rift thus approaching more and more nearly to an east and west direction." The rate of this motion was perhaps the most remarkable circumstance of all. Mr. Baxendell, one of the observers of the rift, and one of our most experienced telescopists, thus describes the changes seen in the belt:—"Since Mr. Long first observed the oblique streak on February 29th, it has gradually extended itself in the direction of the planet's rotation, at the average rate of 3640 miles per day, or 151 miles per hour, the two extremities of the belt remaining constantly on the same parallels of latitude. The belt also became gradually darker and broader."*

Apart from the evidence afforded by this rift respecting

* Two pictures of this belt—as seen March 12th, 1860, and April 9th, 1860—will be found in my article on "Astronomy," in the Encyclopædia Britannica, vol. ii., p. 808.

the swift motions of the cloud-masses enwrapping Jupiter (for a velocity of 151 miles per hour exceeds that of the most tremendous hurricanes on our earth), it has always seemed to me that this one series of observations should suffice of itself to show that the phenomena of Jupiter's cloud-laden atmosphere are not due to solar action. For the rift itself continued, and the changes affecting it continued whether Jovian day was in progress or Jovian night. For one hundred Jovian days or more, and for one hundred Jovian nights, the great cloud-masses on either side of the rift remained in position opposite each other, slowly wheeling, but still continuing face to face, as their equatorial ends rushed onwards at a rate fourfold that of a swift train, even measuring their velocity only by reference to the ends remote from the equator, and regarding these as fixed. Probably the cloud-masses were moving still more swiftly with respect to the surface of the planet below.

Of course, it is just possible that a great dark rift, such as I have described, might appear thus to change in position without any actual transference of the bordering cloud-masses. Mr. Webb, speaking of a number of phenomena, of which those presented by the great rift of 1860 are but a few, says that "they prove an envelope vaporous and mutable like that of the earth, without, however, necessarily inferring" [? implying] "the existence of tempestuous winds: even in our own atmosphere, when near the dew-point, surprising changes sometimes occur very quietly: a cloud-bank observed by Sir J. Herschel, 1827, April 19, was precipitated so rapidly that it crossed the whole sky from east to west at the rate of at least 300 miles per hour; and alterations far more sudden are conceivable where everything is on a gigantic scale." It does not seem to me altogether probable that more rapid alterations would affect cloud-banks covering millions of square miles, than occasionally affect terrestrial cloud-banks covering perhaps a few tens of thousands of square miles; on the contrary, as small terrestrial clouds change relatively in a far more rapid way than large ones, and these than cloud-masses covering a county or a country, so it would seem that the changes affecting our largest cloud-layers would be relatively far more rapid than those affecting cloud-masses which could (many times over) enwrap the whole frame of this earth on which we live. But apart from that, and apart also from the important consideration that all such processes as evaporation and condensation, so far as the sun brings them about should proceed far more sluggishly in the case of a

planet like Jupiter than in that of our earth, which receives some twenty-seven times as much heat from the sun (mile for mile of surface) it is utterly incredible that precipitation should have occurred so steadily and swiftly along one edge of the great rift, and condensation—with such exactly equal steadiness and swiftness—on the opposite edge, that, while the rift as a whole shifted its position during a hundred Jovian nights and days at the rate of 150 miles per hour, its sides should nevertheless remain parallel all the time. Such processes may be spoken of as possible, in the same sense that it is possible that a coin tossed fifty times in succession should always show the same face; but we do not reckon such possibilities among scientific contingencies.

But the motion of great rounded masses in the atmosphere of Jupiter is still more decisive as to the existence not only of a very deep atmosphere, but also as to the swift motions taking place in that atmosphere.

I would, in the first place, note that the very existence of belts in the Jovian atmosphere, and especially of variable belts, implies the great depth at which the real surface of the planet must lie below the visible cloud-layers. Atmospheric belts can only be formed where there are differences of rotational velocity. In the case of our own earth we know that the trade-wind zone and the counter-trade zone owe their origin to the difference of absolute rotational velocity between the equatorial parts of the earth and parts in high latitudes. In the case of Jupiter the difference of this kind is not sufficient to account for the observed belts,—partly because there are many, partly because they are variable, but principally because Jupiter is so much larger than the earth that much greater distances must be traversed in passing from any given latitude to another where the rotational velocity is so many miles per hour more or less. Combining with these considerations the circumstance that the solar action which causes the atmospheric movements from one latitude to another in the case of our earth, is reduced to one twenty-seventh part only of its terrestrial value in the case of Jupiter, we must clearly look to some other cause for the difference of absolute rotational velocity necessary to account for the belts of Jupiter.

Now, it seems to me that we are thus at once led to the conclusion that the cloud-masses forming the belts of Jupiter are affected by vertical currents, uprushing motions carrying them from regions nearer the axis, where the absolute motion due to rotation is slower, to regions farther from the axis, where the motion due to rotation is swifter, and

motions of downrush carrying them from regions of swifter to regions of slower rotational motion. This view seems certainly encouraged by what we find when we come to study more closely the aspect of the Jovian belts. The white spots—some small, some large—which are seen to form from time to time along the chief belts present precisely the appearance which we should expect to find in masses of vapour flung from far down below the visible cloud-surface of Jupiter, breaking their way through the cloud-layers, and becoming visible as they condense into the form of visible vapour in the cooler upper regions of the planet's atmosphere. Then again, the singular regularity with which in certain cases the great rounded white clouds are set side by side, like rows of eggs upon a string, is much more readily explicable as due to a regular succession of uprushes of vapour, from the same region below, than as due to the simultaneous uprush of several masses of vapour from regions set at uniform distances along a belt of Jupiter's surface. The latter supposition is indeed artificial and improbable in the highest degree, and in several distinct respects. It is unlikely that several uprushes should occur simultaneously, unlikely that regions whence uprush took place should be set at equal distances from each other, unlikely that they should lie along the same latitude parallel. On the other hand, the occurrence of uprush after uprush from the same region of disturbance, at nearly uniform intervals of time, is not at all improbable. The rhythmical succession of explosions is a phenomenon, indeed, altogether likely to occur under certain not improbable conditions,—as, for instance, when each explosion affords an excess of relief, if one may so speak, and is therefore followed by a reactionary process, in its turn bringing on a fresh explosion. Now, a rhythmical succession of explosions from the same deep-rooted region of disturbance would produce at the upper level, where we *see* the expelled vapour-masses (after condensation), a series of rounded clouds lying side by side. For each cloud-mass—after its expulsion from a region of slow absolute rotational motion, to a region of swifter motion—would lag behind with reference to the direction of rotational motion. The earlier it was formed the farther back it would lie. Thus each new cloud-mass would lie somewhat in advance of the one expelled next before it; and if the explosions occurred regularly, and with a sufficient interval between each and the next to allow each expelled cloud-mass to lag by its own full length before the next one appeared, there would be seen precisely such a series of

egg-shaped clouds, set side by side, as every careful observer of Jupiter with high telescopic powers has from time to time perceived.*

That these egg-shaped clouds are really egg-shaped—not merely oval in the sense in which a flat elliptic surface is oval—is suggested at once by their aspect. But it is more distinctly indicated when details are examined. It appears to me that considerable interest attaches to some observations which were made by Mr. Brett, in April, 1874, upon some of the rounded spots then visible upon the planet's equatorial zone. It will not be thought that I am disposed, as a rule, to place too much reliance upon the observations and theories of Mr. Brett, seeing that on more than one occasion I have had to call attention to errors into which, in my judgment, he has fallen. For instance, I certainly do not think he has ever seen the solar corona when the sun was not eclipsed, though I have no doubt he saw what he described, which he supposed to be the corona, but which was in reality not the corona. Nor, again, do I accept (though I do not think it worth while to discuss) his theory that Venus has a surface shining with metallic lustre, and is surrounded by a glassy atmosphere; though in that case, again, his description of what he saw may be accepted as it stands, and all that we need reject is his interpretation thereof. In the case of Jupiter's white spots, Mr. Brett's skill as an artist enables us to accept not only his observations, but his interpretation of them, simply because the interpretation depends on artistic, not on scientific, considerations.

"I wish," he says, "to call attention to a particular feature of Jupiter's disc, which" [the feature probably] "appears to me very well defined at the present time, and which seems to afford evidence respecting the physical condition of the planet. The large white patches which occur on and about the equatorial zone, and interrupt the continuity of the dark belts, are well known to all observers, and the particular point in connection with them to which I beg leave to call attention is *that they cast shadows;* that is to

* Webb thus describes the egg-shaped clouds:—"Occasionally the belts throw out dusky loops or festoons, whose elliptical interiors, arranged lengthways and sometimes with great regularity, have the aspect of a girdle of luminous egg-shaped clouds surrounding the globe. These oval forms, which were very conspicuous in the equatorial zone (as the interval of the belts may be termed) in 1869-70, have been seen in other regions of the planet, and are probably of frequent occurrence. The earliest distinct representation of them that I know of is by Dawes, 1851, March 8, but they are perhaps indicated in drawings of the last century."

say, the light patches are bounded on the side farthest from the sun by a dark border shaded off softly towards the light, and showing in a distinct manner that the patches are projected or relieved from the body of the planet. The evidence which this observation is calculated to afford refers to the question whether the opaque body of the planet is seen in the dark belts or the bright ones, and points to the conclusion that it is not seen at all in either of them, but that all we see of Jupiter consists of semi-transparent materials. The particular fact from which this inference would be drawn is, that the dark sides of the suspended or projected masses are not sufficiently hard or sharply defined for shadows falling upon an opaque surface; neither are they sharper upon the light background than upon the dark. The laws of light and shade upon opaque bodies are very simple and very absolute; and one of the most rudimentary of them is that every body has its light, its shade, and its shadow, the relations between which are constant; and that the most conspicuous and persistent edge or limit, in this association of elements, is the boundary of the shadow; the shadow being radically different from the shade, in that its intensity is uniform throughout in any given instance, and is not affected by the form of the surface on which it is cast, whereas the shade is distinguished by attributes of an opposite character. Now, if the dark spaces adjoining the light patches on Jupiter, which I have called shadows, are not shadows at all, but shades, it is obvious that the opaque surface of the planet on which the shadows should fall is concealed; whereas if they are shadows their boundaries are so soft and undefined as to lead to the conclusion that they are cast upon a semi-transparent body, which allows the shadow to be seen indeed, but with diminishing distinctness towards its edge, according to the acuteness of its angle of incidence. Either explanation of the phenomenon may be the true one, but they both lead to the same conclusion, viz., that neither the dark belts nor the bright ones are opaque, and that if Jupiter has any nucleus at all it is not visible to us. [It is obvious that the phenomena I have described would not be visible at the time of the planet's opposition, and the first occasion on which I noticed it was the night of the 16th of April last]."

This reasoning, so far as it relates to the laws of light and shade and shadow, is of course altogether sound. Nor are there any points requiring correction which in any degree affect the astronomical inferences deducible from what Mr. Brett actually saw. I may note that somewhat later

Mr. Knobel observed the shadow of white cloud-masses, and as the shadow had not so much greater a length at that time, two months from opposition, as it had when the planet was much nearer opposition, he infers that the true explanation of the appearance has hardly been found. He appears to have overlooked the fact that the assumption made in the explanation is not that Jupiter has a semi-transparent atmosphere always equally translucent and penetrable to the same depth by the solar rays. When the shadow was shorter than it should have been, had the atmosphere been in the same condition as when Mr. Brett made his observation, it is probable that a layer of clouds interrupted the rays, and thus the shadow was much closer to the cloud-mass throwing it than it would have been had that layer not been there. Mr. Knobel's paper contains very striking evidence of the variability of Jupiter's atmosphere, or rather of the clouds which float in it. "The greater distinctness of the satellites when near the edge," he says, "is a curious phenomenon which has been repeatedly observed by astronomers, but which seems to require explanation." On an occasion described "the second satellite transited a dark limb which was" [seemed] "most dark near the centre, and fainter towards the edge, yet the satellite was almost invisible when on the darkest part of the belt, and was bright and distinct when the background of the belt was faintest." This practically proved that on the occasion in question the dark central part of the belt seemed darker than it really was, by contrast with the bright belts on either side, while the edge seemed lighter than it really was, by contrast with the dark sky on which the planet was projected. In reality the part near the edge must have been darker than the part near the middle, or the satellite could not have appeared brighter when near the edge. No doubt the darkness near the edge (which, by the way, my friend Mr. Browning tested photometrically, and demonstrated, at my suggestion, eight years ago) was due to transparency, the darkness of the sky beyond being to some degree discernible through the edge. But this transparency is not always to be observed to the same degree, or through the same extent of Jovian atmosphere as to depth. Mr. Knobel proceeds, illustrating this the more effectively that he does so unintentionally:—"The third satellite, on March 25, 1874, appeared as a dark spot when in mid-transit, and on nearing the edge appeared as a bright spot without trace of duskiness. But on March 26, 1873" (observe the difference of years), "the fourth satellite made the whole transit as a dark spot,

and was not perceptibly less dark at egress than in mid-transit."

It appears to me demonstrated by the evidence thus far noted, that in a semi-transparent atmosphere of enormous depth, surrounding Jupiter, there float vast cloud-masses, sometimes in layers, at others in irregular heaps, at others having well-rounded forms. These cloud-masses undergo sometimes remarkable changes of shape, often forming or disappearing in a very short time, and thus indicating the inferior activity of the forces at work below them,—in other words, the intense heat of Jupiter's real globe. As to the actual depth of the semi-transparent atmosphere in which these cloud-layers and cloud-masses float, it would be difficult to express an opinion. We do not know how many cloud-layers there are, how thick any cloud-layer may be, how great may be the depth of the vast rounded masses of cloud whose upper surface (that is, the surface remotest from Jupiter's true surface) we can alone see under favourable conditions. But we can indicate a minimum than which the atmosphere's depth is probably not less; and from all the observations which I have examined as bearing on this point, I should be disposed to assign for that minimum at least 6000 miles. I am strongly of opinion that in reality the depth of the Jovian atmosphere is still greater. I cannot doubt that Jupiter has a solid or liquid nucleus, though this nucleus—glowing, as it must be, with a most intense heat—may be greatly expanded; yet I should conceive that, with the enormous attractive power residing in it, containing as it must nearly the whole mass of the planet, its mean density cannot be less than that of the earth. Now a globe of the mass of Jupiter, but of the same mean density as our earth, would have one-fourth of Jupiter's volume—the mean density of Jupiter, as at present judged, being equal to one-fourth that of the earth. The diameter therefore of such a globe would be less than the present diameter of Jupiter in the same ratio that the cube root of unity is less than the cube root of 4, or as 1 is less than 1·5874. Say, roughly, (remembering that the atmosphere of Jupiter must have a considerable mass) the diameter of Jupiter's nucleus would, on the assumptions made, be equal to about five-eighths of his observed diameter, or about 53,000 miles. This is less than his observed diameter by about 22,000 miles, so that the radius of his nucleus would be less than his observed radius by about 11,000 miles, which therefore would be the probable depth of his atmosphere.

But we have still to consider the velocities with which

rounded masses of cloud travel in the very deep atmosphere of Jupiter. "There is clear evidence," I have pointed out in the article 'Astronomy' of the "Encyclopædia Britannica," "that spots on Jupiter are subject to a proper motion like that which affects the spots on the sun. Schmidt, in No. 1973 of the 'Astronomische Nachrichten, gives a number of cases of such proper movements of spots, ranging in velocity from about 7 miles to about 200 miles an hour. It may be noted, also, that from a series of observations of one spot, made between March 13 and April 14, 1873, with the great Rosse reflector, a period of 9 h. 55 m. 4 s. was deduced, while observations of another spot in the same interval gave a rotation period of 9 h. 54 m. 55·4 s." The actual difference of velocity would depend in this case on the actual latitudes of the two spots, which were not micrometrically measured. Taking 200,000 miles as about the circumference of a parallel of latitude passing midway between the spots (only a very rough calculation need be made), we should find that in a period of one rotation, or roughly of ten hours, one spot gained on the other about 51 seconds, or roughly about 1-700th part of a rotation—that is, in distance (dividing 200,000 by 700) about 286 miles in ten hours, or nearly 29 miles an hour.

We have, however, instances of yet greater relative proper motion among cloud-masses. One of these cases I proceed to consider at length.

In June, 1876, two spots were visible upon the disc of Jupiter, so distinct and isolated as to be well adapted for measurement to determine the rate of the planet's rotation. Mr. Brett, observing them first as illustrative of the phenomenon to which he had called attention in 1874, turned his attention afterwards to their rate of motion. He would seem not to have been aware of the fact that the proper motion of bright spots and other markings on Jupiter was already a recognised phenomenon; for he asks whether his "observations of these spots, forming a series extending over a period of 286 hours 20 minutes, afford evidence of proper motion, or whether, on the other hand, they tend to cast any doubt on the accepted rotation of the planet." However, his observations are all the freer from the bias of preconceived opinions. "There were several peculiarities about these two spots," he says, "which seemed to me to give them an eminent claim to attention. They occurred very near to the equator, and were very well defined, and free from entanglement with other markings—an advantage which they have maintained with singular uniformity

throughout the period mentioned ; but the special peculiarity to which attention is asked is that during an interval of five days they remained in the same relative position without any variation whatever. Their stability in respect of latitude during those five days is undoubted ; but the question is whether or not they were equally stable in longitude. This remark only applies to the first five days of the series, because at the end of twelve days a certain deviation was obvious. The distance between the two spots occupied about 42 degrees of Jovian longitude, or about 33,000 miles. Their diameter is nearly equal, being estimated at about one-fourteenth of the planet's diameter, or 6310 miles." The interval of time between these first two observations "was 119 hours,—that is to say, twelve rotations of the planet according to Airy's determination, during which time their distance apart and their latitude remained constant." Between the first and second observations the two spots had gained "44 m. 6 s. in time. Assuming Airy's rotation, viz., 9 h. 55 m. 21 s., the spots have gained on the planet's surface at the rate of 4 m. 2 s. in each revolution."

Between the second observation and the third "there was an interval of seven days, or seventeen rotations of the planet; and the same two spots turn up again somewhat earlier than the calculated time. It unfortunately happens," proceeds Mr. Brett, "that on this occasion their configuration had undergone some change ; but their dimensions and the distance between them remain very much as before. The most important circumstance respecting them is, that their rate of progress shows a certain acceleration." The change, however, in these seven days, is not such as to permit us to believe that the same pair of spots was under observation. *If* so, a change in latitude much more remarkable than the change in longitude had taken place ; for the one which was the most northerly by about 6000 miles at the beginning of the seven days was the most southerly by nearly the same amount at the end of that period. Considering that in the five days between the first and second observations no change of latitude took place, it may fairly be doubted whether a change of the kind and so rapid—amounting, in fact, to nearly 900 miles per day, could have taken place in the interval. Proper motions in latitude may indeed be regarded as not less likely to occur in the case of Jupiter than in that of the sun, where they certainly sometimes occur ; but all the observations hitherto made on Jupiter assure us that, in his case as in the sun's, proper motions in latitude would be very much slower than proper motions in

longitude. We must be content with the evidence of proper motion afforded by the first five days of observation. (The fourth observation only followed the third by about twenty minutes.)

Now, taking this evidence as it stands, and making fair allowance for probable error in an observation of the sort, we may consider that during the 119 hours the two spots were gaining on the estimated rotation-period of the planet by about four minutes per rotation. As they both lie on the equatorial belt, we may take the circuit accomplished by each at about 267,000 miles, or their rate at about 270,000 in ten hours, or 27,000 miles per hour. Hence the distance traversed in four minutes would be about 1800 miles, which would be about the gain per rotation. One-tenth of this, or 180 miles, would be the hourly gain, as compared with the estimated rotation-rate. Mr. Brett takes the least proper motion at 165 miles per hour.

He points out justly that the rotation-rate has been derived from observations of some such spots. So that in reality the only inference we can form is, that the rotation-rate derived from some spots is different from the rotation-rate derived from others, and that some spots (if not all) are certainly not constant in position with respect to the solid nucleus of the planet. That the spots observed by Airy, Mädler, and others should have indicated a slower rate of rotation than those observed by Mr. Brett may fairly be ascribed to the fact that the former were at some distance from the equator, while these last were nearly equatorial. For matter thrown up from the equatorial parts of the true surface of the concealed planet would manifestly differ less in velocity from the superincumbent atmosphere into which they were driven than would masses expelled from higher latitudes. (It is probable that the same explanation applies also in the case of the sun.)

This conclusion, that the spots of Jupiter have rapid rates of relative motion, would of itself be of singular interest, especially when we remember that the larger white spots represent masses of cloud 5000 or 6000 miles in diameter. That such masses should be carried along with velocities so enormous as to change their positions relatively to each other, at a rate sometimes of more than 150 miles per hour, is a startling and stupendous fact. But it appears to me that the fact is still more interesting in what it suggests than in what it reveals. The movements taking place in the deep atmosphere of Jupiter are very wonderful, but the cause of these movements is yet better worthy of study.

We cannot doubt that deep down below the visible surface of the planet—that is, the surface of its outermost cloud-layers—lies the fiery mass of the real planet. Outbursts, compared with which the most tremendous volcanic explosions on our earth are utterly insignificant, are continually taking place beneath the seemingly quiescent envelope of the giant planet. Mighty currents carry aloft great masses of heated vapour, which, as they force their way through the upper and cooler strata of the atmosphere, are converted into visible cloud. Currents of cool vapour descend towards the surface, after assuming no doubt vorticose motions, and sweeping away over wide areas the brighter cloud-masses, so as to form dark spots on the disc of the planet. And owing to the various depths to which the different cloud-masses belong, and whence the uprushing currents of heated vapour have had their origin, horizontal currents of tremendous velocity exist, by which the cloud-masses of one belt or of one layer are hurried swiftly past the cloud-masses of a neighbouring belt or of higher or low cloud-layers. The planet Jupiter, in fact, may justly be described as a miniature sun, vastly inferior in bulk to our own sun, inferior to a greater degree in heat, and in a greater degree yet in lustre, but to be compared with the sun—not with our earth—in size, in heat, and in lustre, and lastly in the tremendous energy of the processes which are at work throughout his cloud-laden atmospheric envelope.

Since the above article was written news has been received by the Astronomical Society that Mr. Todd, a well-known observer of Adelaide, New South Wales, has been able to trace the motions of satellites behind the parts of the planet near the edge, or, in other words, *through* those parts of the planet's atmosphere which have hitherto been regarded as belonging to the mass of the planet itself.

IV. NATIONAL WEALTH AND PUBLIC DEBT.

By Fred. Chas. Danvers.

FEW people, probably, are aware of the vast amount of information that is contained in the several "Command" Papers annually laid before Parliament, and published as Parliamentary Blue Books. Many of them are doubtlessly very dry reading, *per se*, but it is the object of the present article to extract from many hundreds of pages of figures and tabular statements, contained in printed Parliamentary Papers, and from other sources, information on certain points which can hardly fail to interest any "true-born Englishman," and which, being taken or compiled principally from official sources, is not likely to be otherwise than reliable.

It is quite possible to contemplate mentally, in the space of a few moments, on the early prosperity and subsequent decline of Egypt; on the rise and fall of Israel, Assyria, Persia, Macedonia, Greece, and Rome; also on the slow but gradual growth of bands of wandering Scythians and Goths into the mighty empires which now inhabit the North and North-West of Europe, the whole of the North-American Continent, Australasia, and innumerable islands scattered all over the globe. These are all matters of history, which, having been carefully recorded from time to time by cotemporaneous authors, handed down by tradition, or traced by the discovery of monuments or other national emblems, have formed subjects for investigation and study, the results of which are within the reach of every schoolboy. To reflect deliberately, however, upon the circumstances that have led to such violent political changes as have transferred the seat of wealth, learning, and influence from the East to the West, cannot but fill the mind with thoughts calculated to excite feelings of wonderment, if not of awe. Are the changes that have taken place within the past two thousand years due to the action of forces still in existence? and, if so, to what will they ultimately lead? are questions that may very naturally occur to any thinking mind, but it will be by no means easy to afford satisfactory replies to them.

The growth of wealth naturally takes place together with an advance of civilisation in any nation. The former, however, has a natural tendency to encourage luxury and indolence, and to impair that spirit of energy and enterprise

which is necessary to build up a nation. With these prosperity has a tendency to decline, and when once they have become ruling passions in any race its decadence must of necessity speedily follow. To the fact of this Ancient Greece and Rome abundantly testify.

Two of the leading elements of national prosperity are the encouragement of colonisation and foreign trade. Greece and Rome were both of them at one time the pioneers in commerce and in the establishment of foreign settlements. In more modern times Holland, Spain, and Portugal were conspicuous amongst nations for the same reasons, and the names of Sebastian and John Cabot, Tasman, Bartholomew de Diaz, Vasco de Gama, and Columbus are well known to all readers of history, but the lands discovered and taken possession of by them, in the names of their respective Governments, have, for the most part, long since passed into other hands, and the glory and influence once possessed by the countries they represented has become a thing of the past.

In later times the task of colonisation has passed almost exclusively into the hands of the English, and the British flag now waves where formerly a tricolor was the standard representing the supreme power. Emigration from this country, of some description, and on a limited scale, began in the early part of the seventeenth century. Virginia, Massachusetts, the Bermudas, and Barbadoes were the fields in which the first successful attempts at colonisation by the English may be said to have commenced. On the 13th of May, 1607, the first colony in Virginia was planted under a patent granted in the preceding year by James I., to a London company, and subsequently settlements in the Bermudas and Barbadoes were effected in 1612 and 1625. Earlier attempts at founding British colonies had indeed been made by Sir Richard Grenville, Sir Walter Raleigh, and others, but they had failed. To those names, however, must be accorded the honour of having been the pioneers of that progress which has now, in a great measure, secured to England a position never possessed by any other nation.

It can hardly be questioned that the colonial policy of Great Britain during the past 270 years has been mainly instrumental, humanly speaking, in the establishment under the British crown, of the largest empire of either modern or ancient times, compared with which the Roman empire, in the zenith of its prosperity, was insignificant. The possessions of the British empire comprise an area of nearly eight and a half millions of square miles, and a population of upwards of 283 millions, scattered all over the world,

and treading British soil in Europe, Asia, Africa, America, and in most of the principal islands of the seas. The nearest approach to these figures is found in the Russian empire, the area of whose possessions is a very little short of eight millions of square miles, with a population, however, of scarcely 82 millions. Like the British empire, Russia is both an European and Eastern power, but, whilst the latter owns nearly an equal extent of territory, her population is less than one-third of that of the former. China has a smaller territory still, but her population is said to number upwards of 400 millions. There is, however, no official information available on this point, and figures relating to the Flowery Land must therefore be accepted only as estimates, and with caution.

Next in order comes the United States, with 3,600,000 square miles of territory, and 38½ millions of population. After this the territorial possessions of States rapidly decrease. Germany and France are nearly of a size, having each slightly over 200,000 square miles of possessions; but whilst the former has a population of 41 millions, the latter has only 36 millions. Austria and Hungary together comprise about 240,000 square miles, with a united population about equal to that of France. Spain is under 200,000 square miles in extent, and Italy is but 114,000, the populations of these two empires being about 17 millions and 27 millions respectively.

Whilst the British empire thus stands prominently first in extent, it is the only one that has been successful in planting colonies and maintaining them as integral parts of the empire. Those who foreran England in this respect have, as a rule, lost most of the colonial possessions once claimed by them, which have, for the most part, since fallen to the lot of England, whilst those yet retained by them are generally sources of weakness rather than of strength, as a proof of which we may witness Algeria, France's only colony, and Cuba, the principal remnant of Spain's foreign possessions. France has, at different times, resigned to England, either through conquest or by treaty, Canada, Nova Scotia, New Brunswick, Newfoundland, Prince Edward Island, St. Lucia, Grenada, Mauritius, Antigua, Dominica, Tobago, the Bahamas, and Malta. From the Dutch England has obtained the Cape Colony, British Guiana, Ceylon, and St. Helena; whilst from Spain she holds Gibraltar, Trinidad, Jamaica, and the Falkland Islands.

What—it may be asked by some—is the use to us of these colonies? Much, in many ways. It has been

ascertained, beyond doubt, that the English population increases at a more rapid rate than that of any other nation, and, were it not for emigration, and foreign possessions to which our surplus population can safely transfer themselves and their fortunes, the land would long since have become over-populated, and the means of livelihood more difficult by far than it now is. Besides this advantage, however, the command of the colonial trade which this country enjoys *through possession of the colonies*, affords no mean increase to its commercial business, and therefore to its profit. It has been shown, and proved by statistics, by the late Mr. C. W. Eddy, late Secretary to the Colonial Institute, in his "Tables of British Commerce," that "the Trade follows the Flag," or, in other words, that the mother country enjoys the greater portion of the trade of its colonies.

The trade between England and her colonies amounted in 1875 to a total declared value of £161,078,982, of which the exports of foreign and colonial produce, and manufactures from England to British possessions, were valued at £5,562,848; the exports of British and Irish produce at £71,092,163; and the value of imports into England of colonial produce and manufactures at £84,423,971. It will thus be seen that the British colonies do, in a large measure, contribute towards the trade, and therefore the means of wealth, of the mother country. And not only so; for when compared with foreign countries our colonies are very considerable consumers of British produce, as the following figures plainly show:*—

	Consumption of British Produce per Head.			Proportion of British Produce to Total Imports
	£	s.	d.	Per cent.
North American Colonies .	1	5	8	42
Australia and New Zealand .	8	10	3	47
Cape and Natal { Total population .	2	6	4	69
Cape and Natal { White population.	8	12	2	69
West Indies	2	8	7	43
Mauritius	1	14	7	30
United States	0	12	10	31
France	0	6	0	9
Spain	0	2	1	18
Portugal	0	10	4	—
Germany	0	6	11	—

* Hamilton, "On the Colonies." Paper read before the Statistical Society March 19th, 1872.

	Consumption of British Produce per Head. £ s. d.	Proportion of British Produce to Total Imports. Per cent.
Italy	0 4 3	17
Russia	0 0 11	17
Holland	2 16 2	10
Belgium	0 11 10	8
Brazil	0 11 2	—

There are not wanting people who, wholly overlooking the indirect pecuniary benefits to this country derived from the increase of trade secured in consequence of its colonies, and the power and political importance enjoyed by means of its widely-spread possessions, object that they should be in any way a direct pecuniary burden to the mother country. But are they any burden? The supposed cost of the colonies to the Imperial Treasury for the nineteen years 1853 to 1871 was £43,810,000, and during the same time the exports of British produce to the colonies were valued at £450,798,000 (see Archibald Hamilton's paper already referred to). Now the amount realised by the exports of home produce and manufactures constitute in reality so much of our aggregate income; and it has been variously estimated that in this country we are taxed from 10 per cent to as high as 20 per cent on our incomes. Taking the lowest estimate it follows that the Treasury has, during the nineteen years in question, obtained a revenue of £45,000,000 in consequence of the colonial trade, while the expenditure has not exceeded £43,810,000. Similarly it may be shown that for the year 1871—the last year of the above-mentioned nineteen years—the Treasury derived at least £2,580,000 from the colonial trade, and expended, £1,100,000.

Hence we see that England enjoys an increase of wealth, as well as of power and political importance, from the possession of colonies; and this is a source of national prosperity that is not enjoyed by any other nation, the small colonial possessions of Spain, Holland, and France not being of sufficient importance to be taken into account. The colonies and dependencies of Great Britain are, however, more than sixty-seven times in area the size of the United Kingdom.

It would be but little use having extensive colonies unless a large surplus population were forthcoming to people them. England is, of all countries, the best capable of supplying this want, for she is the most productive in increase of population. From some unexplained reason nations vary

greatly as to the relative quickness with which they double themselves, and it has been calculated that in England the population doubles itself in every 56 years; in the New World the Anglo-Saxons double in every 25 years. The Dutch double in 106 years; the Turks in 555 years; the Italians in 135 years; the Swedes in 92 years; the Russians in 100 years; the Spaniards in 112 years; the North Germans in from 50 to 60 years, and the South Germans in 167 years; whilst the French populations take about 140 years in which to double.

According to the last census (1871) the population of the entire British Empire was ascertained to be 234,762,593,* or nearly one-sixth of the total estimated population of the world. The territorial extent of the empire was, at the same time, found to be 7,769,499* square miles, comprising in the two hemispheres, Great Britain and Ireland, the surrounding islands in the British seas, parts of Europe, America, Africa, Asia, and Australasia.

As regards public revenue, the British Empire stands out unmistakably first amongst European nations, whilst the revenue of Great Britain is only exceeded by that of France and of the United States. The gross amount of public revenue of the British Empire amounts to £154,360,336, or nearly 13*s.* 2*d.* per head of population. The revenue of the United Kingdom is £77,131,693, or £2 7*s.* 1*d.* per head of population. The following table shows the same particulars with reference to the principal foreign nations. The figures here given, it must be observed, are exclusive of what may be termed provincial taxation.

	Revenue.	Amount per Head of Population.		
	£.	£	*s.*	*d.*
†France	86,125,625	2	7	8½
United States	77,939,000	2	0	5
Russia (including railways)	72,437,000	0	17	8½
Austria and Hungary	63,356,000	1	15	4
Prussia	53,568,000	1	6	1
Italy	47,226,000	1	15	2¾
Spain	21,502,000	1	5	7¾
Holland (including railways)	8,936,000	2	1	0
Belgium	8,534,000	1	15	3½
Portugal	5,766,000	1	8	8½

* Exclusive of Native States in India, which have been included in the figures before given.

† These figures are for the year 1871. The annual revenue of France is now upwards of one hundred millions.

From these calculations we perceive that the United Kingdom pays more revenue per head of population than any one of the other great powers except France. This alone does not prove that her wealth is greatest, but, taken in conjunction with other considerations which will presently be set forth, it will be seen that in this respect also she is the first amongst nations. The low average of taxation in the entire British Empire is accounted for by the extremely large numbers of her oriental population, which is characteristically poor, especially when compared with the purely British population. The ease with which the national revenues are respectively collected, and the burden borne by the population, show how far each country is living within its powers of taxation; but it will be seen that, whereas the revenues of the British Empire imposed and collected are based upon the necessary expenditure of the State, and prove sufficient —and more so generally—to meet outlay, other States, notwithstanding their relatively lower rates of taxation, fall far short of their requirements.

A close analysis of statistical facts shows that the Anglo-Saxon race are possessed of an elasticity of resources by reason of which they are enabled not only to meet their current expenditure, but they also are equal to any sudden emergency, and are able to bear, when necessary, a considerable increase of taxation, and to meet the requirements of the State caused by any contingency, out of their own resources; besides which they are the bankers of the whole world, and supply funds for the necessities of foreign States also.

With the exception of Russia, Spain, and Italy, the principal European States do manage to maintain an equilibrium in their finances. Austria, however, is sailing very close to the wind, whilst Turkey is hopelessly bankrupt. The power of keeping well within their means appears to belong almost exclusively to the Teutonic races of Northern Europe, if we except France, which, next to England and America, is perhaps really the wealthiest power in existence.

The peculiar situation of England, with reference to the rest of the world, added to her insular position and the possession of many accessible and safe ports, renders her especially well adapted to be the principal merchant and trader for the rest of the world; and by virtue of these and other necessary qualifications she has indeed become the chief carrier of the commerce of nations. The statistics from which we are quoting do not, it is true, show how much of the import and export trade of foreign countries is

carried in English vessels, but from the figures which will presently be given it will be clear that a large portion of the sea-borne trade of foreign countries must be carried in British ships.

The total of imports and exports of merchandise into and from the United Kingdom during the year 1875 amounted in value to £655,551,900, of which the imports were valued at £373,939,577, and the exports at £281,612,323. After England, France stands next highest on the list, her imports and exports having been £176,900,000 and £188,084,000 in value respectively, thus showing trade of an aggregate value of £364,984,000, or only a little more than half that of the United Kingdom. Next comes the United States, with a total trade valued at £240,350,000; then Belgium, with a trade valued at £183,587,000; Austria, with a trade of the value of £152,636,000; and Russia, with only £120,120,000, the value of the import and export trades of other kingdoms being still lower. Thus England, with regard to her imports and exports stands pre-eminently first amongst nations.

Let us now examine these figures, so far as they relate to the English trade, in a little further detail. Of the total imports, those from foreign countries were of the value of £289,515,606, and those from British possessions of the value of £84,423,971. Of this trade, foreign and colonial produce to the value of £58,146,360, or about one-sixth of the whole imported, was re-exported in its original state, whilst by far the greater part of the imports that was not reserved for home consumption, and consisting probably of raw materials, was manufactured in this country and re-exported as British produce. This fact alone will afford some idea of the importance of the manufacturing industries of this country.

Besides the above, however, the bullion trade of this country is by no means inconsiderable. The imports of gold and silver bullion and specie for the year 1875 amounted in value to £33,264,789, whilst the exports for the same time were valued at £27,628,042, from which it would appear that £5,636,747 in bullion and specie remained in the country, in exchange, no doubt, for goods of British production or manufacture of an equal value, which sum therefore appears to represent the balance of trade with foreign countries in favour of this country on the total transactions for the year.

So gigantic a trade as is represented by the above figures must necessarily require a by no means inconsiderable fleet

of vessels for its service. The total tonnage of vessels, of all nationalities and classes, entered and cleared at ports in the United Kingdom during the year, from and to foreign countries and British possessions, was 46,276,838 tons, of which upwards of two-thirds, or 30,944,744 tons, were British vessels.

The mercantile fleet of this country (exclusive of river steamers) consists of 17,221 sailing-vessels, having an aggregate tonnage of 4,044,504 tons, and 2970 steam-vessels, of 1,847,188 tons, or together 20,191 vessels, of a gross aggregate tonnage of 5,891,692 tons, on which are employed nearly 200,000 hands, exclusive of masters. Compared with these figures the mercantile marine of foreign countries is small indeed. Next after Great Britain the United States have the largest fleet. The vessels employed upon foreign trade aggregate 3,272,739 tons, and those employed in the river, lake, and home trade 1,423,288 tons, or together 4,696,027 tons. Next comes Norway, with 1,245,293 tons; then France, with 1,068,031 tons: Italy, with 1,046,439 tons, whilst other foreign countries have none of them more than about half the last-named tonnage each, and most of them considerably less. The tonnage of British merchant vessels is in excess of those of Norway, Sweden, France, Italy, Austria, Holland, Denmark, Hamburg, Bremen, Belgium, and Greece all put together.

Perhaps one of the most important sources of wealth to a nation is its mineral production. It is not intended now to compare the relative produce of different countries in coal and iron—the two most important products of mining industries; but we may notice a few circumstances in connection with the imports and exports of different countries. If we except the United States, France and Belgium alone of foreign countries possess any iron and coal industries of importance, whilst these form two of the most important industries of Great Britain; but gold, silver, and other more valuable metals abound chiefly abroad, and are imported into this country in their raw state. The exports of foreign countries consist chiefly of food supplies and raw materials, whilst of those from the United Kingdom manufactured articles largely predominate; and thus we see that England not only acts as the principal carrier, but also as the chief manufacturer for other nations; hence the wealth of the latter has a tendency to flow to this country.

Not only is coal produced in England in larger quantities than in any other part of the world, but there is also a general distribution of that mineral throughout the

possessions of the British Empire abroad; and when we consider how important coal is for the maintenance of supremacy on the seas, this appears to be a most important element both for commercial and naval success. As was observed by Mr. Alfred Eccles, in his Report on the New Zealand Exhibition of 1865, "It is a fortunate circumstance that, with few exceptions, wherever important British colonies have been founded, there has also been found a local supply of coal; thus the colonist of that race which, above all others, has attained great national prosperity by means of its vast mineral wealth, find in their new countries the same agents whereby to build up a like greatness." And, with regard to its importance to our steam navy and merchant vessels, the late Mr. Eddy, to whom we have already referred, has remarked—"Here, again, the singular fact which I have already referred to meets us, viz., that of the general absence of true coal fit for marine purposes, and also conveniently situated near the coast, from all except British territory. The fact is not only remarkable, but significant of the destiny of our race as the great maritime power of the world. During the whole voyage from England to India, up the Mediterranean, the Red Sea, and along the coast of Arabia, no native coal appears, as far as I am aware; and moreover, whenever you come to depôts of fuel you find it is of English origin, and generally, too, on English territory. Witness Gibraltar, Malta, Aden. Verily we hold the keys of the world, and long may we continue to hold them with a firm grasp. Until we reach India we meet with none but English coal, and there British coal from our own colonies at the Antipodes meets ours in the market."

Turning now to the national debts of different countries, it appears that Holland was the first to introduce the practice of raising public loans, and other nations have not been slow to follow the example. The public debt of this country may be said to date from the reign of James I., and, like other countries, it has chiefly been raised for the purpose of defraying the expenses of wars and of military preparations, although in more recent years many nations have added to their debts for the construction of railways, but these, in many instances, have been made more with a view to military and political than to commercial and peaceful requirements, present or prospective. It is perhaps a curious coincidence that England and France, the two most prosperous countries in Europe, have also the largest debts, exceeding in both cases upwards of £700,000,000 sterling, and they are also the only two upon whom the burden of

paying interest falls comparatively light. As regards England, her debt may be said to be entirely an "internal" debt, all monies required by the State being readily obtained at home. It is not so, however, with other countries, whose "internal" debts are, for the most part, of small amount, whilst the greater part of their loans have been obtained from this country. England, France, and the United States alone are enabled to take steps for the gradual extinction of their debts, whilst many other countries are obliged to raise fresh loans annually to meet their requirements for current expenses, or, failing that, to repudiate obligations to their bondholders, of the truth of which circumstance too many Englishmen have bitter cause of experience.

With regard to the amounts of public debts the following figures furnish such particulars as are obtainable. Column 1 shows the total public debts as they are given in the printed Parliamentary Papers, whilst Column 2 shows the amounts of the several external debts given in the "Investor's Monthly Manual," and which are quoted in the London Stock Exchange List :—

	£.	£.
Russia	296,096,000	146,549,128
Norway	1,766,000	—
Sweden	6,681,000	3,958,100
Denmark . . .	12,899,000	1,804,000
Germany . . .	140,404,000	—
Holland	*79,678,000	*79,801,240
Belgium	43,220,000	27,270,000
France	*731,338,000	*756,741,160
Spain	347,012,000	165,506,300
Portugal . . .	77,668,000	77,304,710
Switzerland . .	1,104,000	—
Austria	264,487,000	199,420,132
Hungary . . .	53,269,000	23,062,200
Italy	339,051,000	33,807,582
Greece	17,015,000	4,749,900
United States . .	469,427,000	338,576,803

If we attempt to draw any conclusion from the foregoing facts and figures, it is impossible to do otherwise than admit that England's supremacy is undeniable. That it is

* There is evidently some discrepancy in these figures, due either to typographical errors or to imperfect information on the part of the compilers of the respective returns from which the figures are taken.

acknowledged by other nations is conspicuous from the importance attached by them to her counsel and advice in political affairs. Wealth in individuals always has, and always will, command respect from its poorer neighbours, and what is true in individuals also holds good with regard to nations. At no time, probably, has the influence of England been more proved than recently. England is not only the greatest European power, but as an Eastern power, also, she holds a higher position than any hitherto recorded in history. The Macedonian and Roman Empires of old were small compared with that of Great Britain of the present day, and, so far as can be at present seen, her power and influence seem rather destined to increase than to diminish, whilst English is not only cultivated by other nations to an extent far beyond all past experience with regard to the Latin language,—formerly the most popular of tongues,—but it has been calculated that, at the present rate at which it is being spread, English will, a hundred years hence, be spoken by greater numbers than, according to the most reliable estimates, there are now people existing upon the face of the earth.

A glance at a telegraph map of the world will at once show how almost all the submarine cables connect British possessions, extending from New Zealand through Australia, the Straits Settlements, India, Aden, Malta, Gibraltar, to England, Ireland, and thence across the Atlantic to Newfoundland and Canada, by which all our most important possessions are placed in telegraphic communication with each other, whilst branch lines provide means of communication with other British territories and with most parts of the civilised world. These cables are all British property: and as they are required chiefly for commercial purposes, they are very properly possessed by that nation which claims the largest part of the commerce of the world.

V. "THE GREAT ICE AGE" AND THE ORIGIN OF THE "TILL."

By W. Mattieu Williams, F.R.A.S., F.C.S.

THE growth of Science is becoming so overwhelming that the old subdivisions of human knowledge are no longer sufficient for the purpose of dividing the labour of experts. It is scarcely possible now for any man to become a naturalist, a chemist, or a physicist, in the full sense of either term; he must, if he aims at thoroughness, be satisfied with a general knowledge of the great body of science, and a special and full acquaintance with only one or two of its minor subdivisions. Thus geology, though but a branch of natural history, and the youngest of its branches, has now become so extensive that its ablest votaries are compelled to devote their best efforts to the study of sections which but a few years ago were scarcely definable.

Glaciation is one of these, which now demands its own elementary text-books over and above the monographs of original investigators. This demand has been well supplied by Mr. James Geikie in "The Great Ice Age,"* of which a second edition has just been issued. Every student of glacial phenomena owes to Mr. Geikie a heavy debt of gratitude for the invaluable collection of facts and philosophy which this work presents. It may now be fairly described as a standard treatise on the subject which it treats.

One leading feature of the work offers a very aggressive invitation to criticism. Scotchmen are commonly accused of looking upon the whole universe through Scotch spectacles, and here we have a Scotchman treating a subject which affects nearly the whole of the globe, and devoting about half of his book to the details of Scottish glacial deposits; while England has but one-third of the space allowed to Scotland, Ireland but a thirtieth, Scandinavia less than a tenth, North America a sixth, and so on with the rest of the world. Disproportionate as this may appear at first glance, further acquaintance with the work justifies the pre-eminence which Mr. Geikie gives to the Scotch glacial deposits. Excepting Norway, there is no country in Europe which affords so fine a field for the study of the vestiges of extinct glaciers as Scotland, and Scotland has

* The Great Ice Age, and its Relation to the Antiquity of Man. By James Geikie, F.R.S., &c. Second edition, revised, 1877. Daldy and Isbister.

an advantage even over Norway in being much better known in geological detail. Besides this, we must always permit the expounder of any subject to select his own typical illustrations, and welcome his ability to find them in a region which he himself has directly explored.

Mr. Geikie's connection with the Geological Survey of Scotland has afforded him special facilities for making good use of Scottish typical material, and he has turned these opportunities to such excellent account that no student after reading "The Great Ice Age" will find fault with its decided nationality.

The leading feature—the basis, in fact—of this work deserves especial notice, as it gives it a peculiar and timely value of its own. This feature is that the subject—as compared with its usual treatment by other leading writers—is turned round and presented, so to speak, bottom upwards. De Saussure, Charpentier, Agassiz, Humboldt, Forbes, Hopkins, Whewell, Stark, Tyndall, &c., have studied the living glaciers, and upon the data thus obtained have identified the work of extinct glaciers. Chronologically speaking they have proceeded backwards, a method absolutely necessary in the early stages of the enquiry, and which has yielded admirable results. Geikie, in the work before us, proceeds exactly in the opposite order. Availing himself of the means of identifying glacial deposits which the retrogressive method affords, he plunges at once to the lowest and oldest of these deposits, which he presents the most prominently, and then works upwards and onwards to recent glaciation.

The best illustration I can offer of the timely advantage of this reversed treatment is (with due apology for necessary egotism) to state my own case. In 1841, when the "glacial hypothesis," as it was then called, was in its infancy, Prof. Jamieson, although very old and nearly at the end of his career, took up this subject with great enthusiasm, and devoted to it a rather disproportionate number of lectures during his course on Natural History. Like many of his pupils I became infected by his enthusiasm, and went from Edinburgh to Switzerland, where I had the good fortune to find Agassiz and his merry men at the "*Hotel des Neufchatelois*"—two tents raised upon a magnificent boulder floating on the upper part of the Aar glacier.[1] After a short but very active sojourn there I "did," not without physical danger, many other glaciers in Switzerland and the Tyrol, and afterwards practically studied the subject in Norway, North Wales, and, wherever else an opportunity offered;

reading in the meantime much of its special literature; but, like many others, confining my reading chiefly to authors who start with living glaciers and describe their doings most prominently. When, however, I read the first edition of Mr. Geikie's "Great Ice Age," immediately after its publication, his mode of presenting the phenomena, bottom upwards, suggested a number of reflections that had never occurred before, leading to other than the usual explanations of many glacial phenomena, and correcting some errors into which I had fallen in searching for the vestiges of ancient glaciers. As these suggestions and corrections may be interesting to others, as they have been to myself, I will here state them in outline.

The most prominent and puzzling reflection or conclusion suggested by reading Mr. Geikie's description of the glacial deposits of Scotland was, that the great bulk of them are quite different from the deposits of existing glaciers. This reminded me of a previous puzzle and disappointment that I had met in Norway, where I had observed such abundance of striation, such universality of polished rocks and rounded mountains, and so many striking examples of perched blocks, with scarcely any decent vestiges of moraines. This was especially the case in Arctic Norway. Coasting from Trondhjem to Hammerfest, winding round glaciated islands, in and out of fjords banked with glaciated rock-slopes, along more than a thousand miles of shore line, displaying the outlets of a thousand ancient glacier valleys, scanning eagerly throughout from sea to summit, landing at several stations, and climbing the most commanding hills, I *saw only one ancient moraine*—that at the Oxfjord Station described in Chap. 6 of "Through Norway with a Knapsack."

But this negative anomaly is not all. The ancient glacial deposits are not only remarkable on account of the absence of the most characteristic of modern glacial deposits, but in consisting mainly of something which is quite different from any of the deposits actually formed by any of the modern glaciers of Switzerland or any other country within the temperate zones.

I have seen nothing either at the foot or the sides of any living Alpine or Scandinavian glacier that even approximately represents the "till" or "boulder clay," and have met with no note of this very suggestive anomaly by any writer on glaciers. Yet these are vast deposits, covering thousands of square miles even of the limited area of the British Isles, and which constitute the main evidence upon which we base all our theories respecting the existence and the vast extent and influence of the "Great Ice Age."

Although so different from anything at present produced by the Alpine or Scandinavian glaciers, this great deposit is unquestionably of glacial origin. The evidences upon which this general conclusion rests are fully stated by Mr. Geikie, and may safely be accepted as incontrovertible. Whence, then, the great difference ?

One of the suggestions to which I have already alluded as afforded by reading Mr. Geikie's book was a hypothetical solution of this difficulty, but the verification of this hypothesis demanded a re-visit to Norway. An opportunity for this was afforded in the summer of 1874, during which I travelled round the coast from Stavanger to the Arctic frontier of Russia, and through an interesting inland district. The observations there made, and strengthened by subsequent reflections, have so far confirmed my original speculative hypothesis that I now venture to state it briefly as follows :—

That the period appropriately designated by Mr. Geikie as the " Great Ice Age " includes at least two distinct periods or epochs—the first of very great intensity or magnitude, during which the Arctic regions of our globe were as completely glaciated as the Antarctic now are, and the British islands and a large portion of Northern Europe were glaciated as completely, and nearly in the same manner, as Greenland is at the present time ; that long after this, and immediately preceding the present geological epoch, there was a minor glacial period, when only the now existing valleys favourably shaped and situated for glacial accumulations were partially or wholly filled with ice. There may have been many intermediate fluctuations of climate and glaciation, and probably were such, but as these do not affect my present argument they need not be here considered.

So far I agree with the general conclusions of Mr. Geikie as I understand them, and with the generally received hypotheses, but in what follows I have ventured to diverge materially.

It appears to me that the existing Antarctic glaciers and some of the glaciers of Greenland are essentially different in their conformation from the present glaciers of the Alps, and those now occupying some of the fjelds and valleys of Norway ; and that the glaciers of the earlier or greater glacial epoch were similar to those now forming the Antarctic barrier, while the glaciers of the later or minor glacial epoch resembled those now existing in temperate climates, or were intermediate between these and the Antarctic glaciers. The nature of the difference which I suppose to

exist between the two classes of glaciers is this:—The glaciers (properly so called) of temperate climates being the overflow of the *nevé*, or the ice which is protruded below the snow-line, into the region where the summer thaw exceeds the winter snow-fall, is necessarily subject to continual thinning or wasting from its upper or exposed surface, and thus finally becomes liquefied, and is terminated by direct solar action.

Many of the characteristic phenomena of Alpine glaciers depend upon this; among the more prominent of which are the superficial extrusion of boulders or rock fragments that have been buried in the *nevé* or have fallen into the crevasses of the upper part of the true glacier, and the final deposit of these same boulders or fragments at the foot of the glaciers forming ordinary moraines.

But this is not all. The thawing which extrudes, and finally deposits the larger fragments of rock, sifts from them the smaller particles, the aggregate bulk of which usually exceeds very largely that of the larger fragments. This fine silt or sand thus washed away is carried by the turbid glacier torrent to considerable distances, and deposited as an alluvium wherever the agitated waters find a resting-place.

Thus the *débris* of the ordinary modern glacier is effectively separated into two or more very distinct deposits, the moraine at the glacier foot consisting of rock fragments of considerable size, with very little sand or clay or other fine deposit between them, and a distant deposit of totally different character, consisting of gravel, sand, clay, or mud, according to the length and conditions of its journey. The "chips," as they have been well called, are thus separated from what I may designate the *filings* or *sawdust* of the glacier.

The filings from the existing glaciers of the Bernese Alps are gradually filling up the lake basins of Geneva and Constance, repairing the breaches made by the erosive action of their gigantic predecessors; those of the southern slope of the Alps are doing a large share in filling up the Adriatic; while the chips of all merely rest upon the glacier beds forming the comparatively insignificant terminal moraine deposits.

The same in Scandinavia. The Storelv of the Jostedal is fed by the melting of the Krondal, Nygaard, Bjornestegs, and Soldal glaciers. It has filled up a branch of the deep Sogne fjord, forming an extensive fertile plain at the mouth of its wild valley, and is depositing another subaqueous

plain beyond, while the moraines of the glaciers are but inconsiderable and comparatively insignificant heaps of loose boulders, spread out on the present and former shores of the above-named glaciers, which are overflows from one side of the great *nevé*, the Jostedal Sneefornd. All of these glaciers flow down small lateral valleys, spread out, and disappear in the main valley, which has now no glacier of its own, though it was formerly glaciated throughout.

What must have been the condition of this and the other great Scandinavian valleys when such was the case? To answer this question rationally we must consider the meteorological conditions of that period. Either the climate must have been much colder or the amount of precipitation vastly greater than at present, in order to produce the general glaciation that rounded the mountains up to a height of some thousands of feet above the present sea-level. Probably both factors co-operated to effect this vast glaciation, the climate colder, and the snow-fall also greater. The whole of Scandinavia, or as much as then stood above the sea, must have been a *nevé* or sneefornd on which the annual snow-fall exceeded the annual thaw.

This is the case at present on the largest *nevé* of Europe, the 500 square miles of the great plateau of the Jostedals and Nordfjords Sneefornd; on all the overflowing *nevé* or snow-fields of the Alps above the snow-line, over the greater part of Greenland, and (as the structure of the southern icebergs prove) everywhere within the great Antarctic ice barrier.

What, then, must happen when the snow-line comes down, or nearly down, to the sea-level? It is evident that the outthrust glaciers, the overflow down the valleys, cannot come to an end like the present Swiss and Scandinavian glaciers, by the direct melting action of the sun. They may be somewhat thinned from below by the heat of the earth, and that generated by their own friction on the rocks, but these must be quite inadequate to overcome the perpetual accumulation due to the snow-fall upon their own surface and the vast overflow from the great snow-fields above. They must go on and on, ever increasing, until they meet some new condition of climate or some other powerful agent of dissipation—something that can effectively melt them.

This agent is very near at hand in the case of the Scandinavian valleys and those of Scotland. It is the sea. I think I may safely say that the valley glaciers of these countries during the great ice age *must* have reached the

sea and there terminated their existence, just as the Antarctic glaciers terminate at the present Antarctic ice-wall.

What must happen when a glacier is thus thrust out to sea? This question is usually answered by assuming that it slides along the bottom until it reaches such a depth that flotation commences, and then it breaks off or "calves" as icebergs. This view is strongly expressed by Mr. Geikie (p. 47) when he says that—"The seaward portion of an Arctic glacier cannot by any possibility be floated up without sundering its connection with the frozen mass behind. So long as the bulk of the glacier much exceeds the depth of the sea, the ice will of course rest upon the bed of the fjord or bay without being subjected to any strain or tension. But when the glacier creeps outwards to greater depths, then the superior specific gravity of the sea-water will tend to press the ice upward. That ice, however, is a hard continuous mass, with sufficient cohesion to oppose for a time this pressure, and hence the glacier crawls on to a depth far beyond the point at which, had it been free, it would have risen to the surface and floated. If at this great depth the whole mass of the glacier could be buoyed up without breaking off, it would certainly go to prove that the ice of Arctic regions, unlike ice anywhere else, had the property of yielding to mechanical strain without rupturing. But the great tension to which it is subjected takes effect in the usual way, and the ice yields, not by bending and stretching, but by breaking." Mr. Geikie illustrates this by a diagram showing the "calving" of an iceberg.

In spite of my respect for Mr. Geikie as a geological authority, I have no hesitation in contradicting some of the physical assumptions included in the above.

Ice has no such rigidity as here stated. It does possess in a high degree "the property of yielding to mechanical strain without rupturing." We need not go far for evidence of this. Everybody who has skated or seen others skating on ice that is but just thick enough to "bear" must have felt or seen it yield to the mechanical strain of the skater's weight. Under these conditions it not only bends under him, but it afterwards yields to the reaction of the water below, rising and falling in visible undulations, demonstrating most unequivocally a considerable degree of flexibility. It may be said that in this case the flexibility is due to the thinness of the ice; but this argument is unsound, inasmuch as the manifestation of such flexibility does not depend upon absolute thickness or thinness, but upon the

relation of thickness to superficial extension. If a thin sheet of ice can be bent to a given arc, a thick sheet may be bent in the same degree, but the thicker ice demands a greater radius and proportionate extension of circumference. But we have direct evidence that ice of great thickness—actual glaciers—may bend to a considerable curvature before breaking. This is seen very strikingly when the uncrevassed ice-sheet of a slightly inclined *nevé* suddenly reaches a precipice and is thrust over it. If Mr. Geikie were right, the projecting cornice thus formed should stand straight out, and then, when the transverse strain due to the weight of this rigid overhang exceeded the resistance of tenacity, it should break off short, exposing a face at right angles to the general surface of the supported body of ice. Had Mr. Geikie ever seen and carefully observed such an overhang or cornice of ice, I suspect that the above quoted passage would not have been written.

Some very fine examples of such ice-cornices are visible from the ridge separating the Handspikjen Fjelde from the head of the Jostedal, where a fine view of the great *nevé* or sneefornd is obtained. This side of the *nevé* terminates in precipitous rock-walls; at the foot of one of these is a dreary lake, the Styggevand. The overflow of the *nevé* here forms great bending sheets that reach a short way down, and then break off and drop as small icebergs into the lake.*

The ordinary course of glaciers afford abundant illustrations of the plasticity of such masses of ice. It spreads out where the valley widens, contracts where the valley narrows, and follows all the convexities or concavities of the axial line of its bed. If the bending thus enforced exceeds a certain degree of abruptness crevasses are formed, but a considerable bending occurs before the rupture is effected, and crevasses of considerable magnitude are commonly formed without severing one part of a glacier from another. They are usually V-shaped, in vertical section, and in many the rupture does not reach the bottom of the glacier. Very rarely indeed does a crevass cross the whole breadth of a glacier in such a manner as to completely separate, even temporarily, the lower from the upper part of the glacier.

If a glacier can thus bend *downwards* without "sundering its connection with the frozen mass behind," surely it may bend upwards in a corresponding degree, either with or

* *See* "Through Norway with a Knapsack," chapters 11 and 12, for further descriptions of these.

without the formation of crevasses, according to the thickness of the ice and the degree of curvature.

A glacier reaching the sea by a very steep incline would probably break off, in accordance with Mr. Geikie's description, just as an Alpine glacier is ruptured fairly across when it makes a cascade over a suddenly precipitous bend of its path. One entering the sea at an inclination somewhat less precipitous than the minor limit of the effective rupture gradient it would be crevassed in a contrary manner to the crevassing of Alpine glaciers. Its crevasses would gape downwards instead of upwards—have an A-shaped instead of a V-shaped section.

With a still more moderate slope, the up-floating of the termination of the glacier, and a concurrent general uplifting or upbending of the submerged portion of the glacier might occur without even a partial rupture or crevasse formation occurring.

Let us now follow out some of the necessary results of these conditions of glacier existence or glacial prolongation. The first and most notable, by its contrast with ordinary glaciers, is the absence of lateral, medial, or terminal moraines. The larger masses of *débris*, the chippings that may have fallen from the exposed escarpments of the mountains upon the surface of the upper regions of the glacier, instead of remaining on the surface of the ice and standing above its general level by protecting to some extent the ice on which they rest from the general snow-thaw, would become buried by the upward accretion of the ice due to the unthawed stratum of each year's snow-fall.

The only thinning agency at work upon such glaciers during their journey over the *terra firma* being the outflow of terrestrial heat, and that due to their friction upon their beds, the thinning must all take place from below, and thus, as the glacier proceeds downwards, these rock fragments must be continually approaching the bottom instead of continually approaching the top, as in the case of modern Alpine glaciers flowing below the snow-line.

An important consequence of this must be that the erosive power of these ancient glaciers was, *cæteris paribus*, greater than that of modern Alpine glaciers, especially if we accept those theories which ascribe an actual internal growth or regeneration of glaciers by the relegation below of some of the water resulting from the surface-thaw.

It follows, therefore, that such glaciers could not deposit any moraines such as are in course of deposition by existing Alpine and Scandinavian glaciers.

What, then, must become of the chips and filings of these outfloating glaciers? They must be carried along with the ice *so long as that ice rests upon the land*; for this *débris* must consist partly of fragments imbedded in the ice, and partly of ground and re-ground excessively subdivided particles, forming a slimy mud that must either cake into what I may call ice-mud, and become a part of the glacier, or flow as liquid mud or turbid water beneath it, as with ordinary glaciers. The quantity of water being relatively small under the supposed conditions, the greater part would be carried forward to the sea by the ice rather than by the water.

As the glacier with its lower accumulation advanced into deeper and deeper water, its pressure upon its bed must progressively diminish until it reaches a line where it would just graze the bottom with a touch of feathery lightness. Somewhere before reaching this it would begin to deposit its burden on the sea-bottom, the commencement of this deposition being determined by the depth whereat the tenacity of the deposit, or its friction against the sea-bottom, or both combined, becomes sufficient to overpower the now-diminished pressure and forward thrusting, or erosive power of the glacier.

Farther forward, in deeper water, where the ice becomes fairly raised above the original sea-bottom, a rapid thawing must occur by the action of the sea-water, and if any communication existed between this ice-covered sea and the waters of warmer latitudes a further thawing must result from the currents that would necessarily be formed by the interchange of water of varying specific gravities. Deposition would thus take place in this deeper water, continually shallowing it or bringing up the sea-bottom nearer to the ice-bottom.

This raising of the sea-bottom must occur not only here, but farther back, *i.e.*, from the line at which any deposition commenced. This line or region, whereat the depth is just sufficient to allow the ice to rest lightly on its own deposit and slide over it without either sweeping it forward or depositing any more upon it, becomes an interesting critical region, subject to continuous forward extension during the lifetime of the glacier, as the deposition beyond it must continually raise the sea-bottom until it reaches this critical depth at which this deposition must cease. This would constitute what I may designate the normal depth of the glaciated sea, or the depth to which it would be continually tending, during a great glacial epoch, by the formation of a submarine bank or plain of glacier deposit, over which the

FIG. 1.—LOW GLACIATED ROCKS OF THE THRENEN ISLANDS, NEAR THE COAST.

FIG. 2.—OUTLYING LOFODENS, NOT GLACIATED.

glacier would slide without either grinding it lower by erosion or raising it higher by deposition.

But what must be the nature of this deposit? It is evident that it cannot be a mere moraine consisting only of the larger fragments of rock such as are now deposited at the foot of glaciers that die out before reaching the sea. Neither can it correspond to the glacial silt which is washed away and separated from these larger fragments by glacial streams, and deposited at the outspreadings of glacier torrents and rivers. It will correspond to neither the assorted gravel, sand, or mud of these alluvial deposits, but must be an agglomeration of all the infusible solid matter the glacier is capable of carrying.

It must contain, in heterogeneous admixture, the great boulders, the lesser rock fragments, the gravel chips, the sand, and the slimy mud; and these settling down quietly in the cold, gloomy waters, overshadowed by the great ice-sheet, must form just such an agglomeration as we find in the boulder clay and tills; and lie just in those places where these deposits abound, provided the relative level of land and sea during the glacial epoch were suitable.

I should make one additional remark relative to the composition of this deposit, viz., that under the conditions supposed, the original material detached from the rocks around the upper portions of the glaciers would suffer a far greater degree of attrition at the glacier bottom than it obtains in modern Alpine glaciers, inasmuch as in these it is removed by the glacier torrent when it has attained a certain degree of fineness, while in the greater glaciers of the glacial epoch it would be carried much further in association with the solid ice, and be subjected to more grinding and re-grinding against the bottom. Hence a larger proportion of slimy mud would be formed, capable of finally indurating into stiff clay such as forms the matrix of the till and boulder clay.

The long journey of the bottom *débris* stratum of the glacier, and its final deposition when in a state of neutral equilibrium between its own tendency to repose and the forward thrust of the glacier, would obviously tend to arrange the larger fragments of rock in the manner in which they are found imbedded in the till, *i.e.*, the oblong fragments lying with their longer axes and their best marked striæ in the direction of the motion of the glacier. The "striated pavements" of the till are thus easily explained, as the surface upon which the ice advanced when its depo-

sits had reached the critical or neutral height. Such a pavement would continually extend outwards.

The only sorting of the material likely to occur under these conditions would be that due to the earlier deposition and entanglement of the larger fragments, thus producing a more stony deposit nearer inland, just as Mr. Geikie describes the actual deposits of till where, "generally speaking, the stones are most numerous in the till of hilly districts; while at the lower levels of the country the clayey character of the mass is upon the whole more pronounced." These "hilly districts," upon the supposition of greater submergence, would be the near shore regions, and the lower levels the deeper sea where the glacier floated freely.

The following is Mr. Geikie's description of the distribution of the till (page 13):—"It is in the lower-lying districts of the country where till appears in greatest force. Wide areas of the central counties are covered up with it continuously, to a depth varying from two or three feet up to one hundred feet and more. But as we follow it towards the mountain regions it becomes thinner and more interrupted —the naked rock ever and anon peering through, until at last we find only a few shreds and patches lying here and there in sheltered hollows of the hills. Throughout the Northern Highlands it occurs but rarely, and only in little isolated patches. It is not until we get away from the steep rocky declivities and narrow glens and gorges, and enter upon the broader valleys that open out from the base of the highland mountains to the low-lying districts beyond, that we meet with any considerable deposits of stony clay. The higher districts of the Southern Uplands are almost equally free from any covering of till."

This description is precisely the same as I must have written, had I so far continued my imaginary sketch of the results of ancient glaciation as to picture what must remain after they had all melted away, and the sea had receded sufficiently to expose their submarine deposits.

Throughout the above I have assumed a considerable submergence of the land as compared with the present sea-level on the coasts of Scotland, Scandinavia, &c.

The universality of the terraces in all the Norwegian valleys opening westward proves a submergence of *at least* 600 or 700 feet. When I first visited Norway, in 1856, I accepted the usual description of these as alluvial deposits; was looking for glacial vestiges in the form of moraines, and thus quite failed to observe the true nature of these vast accumulations, which was obvious enough when I

re-examined them in the light of more recent information. Some few are alluvial, but they are exceptional and of minor magnitude. As an example of such alluvial terraces I may mention those near the mouth of the Romsdal, that are well seen from the Aak Hotel, and which a Russian prince, or other soldier merely endowed with military eyes, might easily mistake for artificial earthworks erected for the defence of the valley.

In this case, as in the others where the terraces are alluvial, the valley is a narrow one, occupied by a relatively wide river loaded with recent glacial *débris*. It evidently filled the valley during the period of glacial recession.

The ordinary wider valleys, with a river that has cut a narrow channel through the outspread terrace-flats, display a different formation. Near the mouth of such valleys I have seen cuttings of more than a hundred feet in depth, through an unbroken terrace of most characteristic till, with other terraces rising above it. This is the ordinary constitution of the *lower portions* of most of the Scandinavian terraces.

These terraces are commonly topped with quite a different stratum, which at first I regarded as a subsequent alluvial or esturine deposit, but further examination suggested another explanation of the origin of some portions of this superficial stratum, to which I shall refer hereafter.

These terraces prove a rise of sea or depression of land, during the glacial epoch, to the extent of 600 feet as a *minimum*, while the well-known deposits of Arctic shells at Moel Tryfaen and the accompanying drift have led Prof. Ramsay to estimate "the probable amount of submergence during some part of the glacial period at about 2300 feet."*

It would be out of place here to reproduce the data upon which geologists have based their rather divergent opinions respecting the actual extent of the submergence of the western coast of North Europe. All agree that a great submergence occurred, but differ only as to its extent, their estimates varying between 1000 and 3000 feet.

There is one important consideration that must not be overlooked, viz., that—if my view of the submarine origin of the till be correct—the mere submergence of the land at the glacial period does not measure the difference between the depth of the sea at that and the present time, seeing that the deposits from the glaciers must have shallowed it very materially.

* Lyell, Elements of Geology, p. 159.

It is only after contemplating thoughtfully the present form of the granitic and metamorphic hills of Scandinavia,—hills that are always angular when subjected only to subaërial weathering,—that one can form an adequate conception of the magnitude of this shallowing deposit. The rounding, shaving, grinding, planing, and universal abrasion everywhere displayed appear to me to justify the conclusion that if the sea were now raised to the level of the terraces, *i.e.*, 600 feet higher than at present, the mass of matter abraded from the original Scandinavian mountains, and lying under the sea, would exceed the whole mass of mountain left standing above it.

The first question suggested by reading Mr. Geikie's book was whether the terraces are wholly or partially formed of till, and more especially whether their lower portions are thus composed. This, as already stated, was easily answered by the almost unanimous reply of all the many Norwegian valleys I traversed. Any tourist may verify this. The next question was whether this same till extends below the sea. This was not so easily answered by the means at my disposal, as I travelled hastily round the coast from Stavanger via the North Cape to the frontier of Russian Lapland in ordinary passenger steam packets, which made their stoppages to suit other requirements than mine. Still I was able to land at many stations, and found that wherever there was a gently sloping strand at the mouth of an estuary, or of a valley whose river had already deposited its suspended matter (a common case hereabouts, where so many rivers terminate in long estuaries or open out into bag-shaped lakes near the coast), and where the bottom had not been modified by secondary glaciation, that the receding tide displayed a sea bottom of till, covered with a thin stratum of loose stones and shells. In some cases the till was so bare that it appeared like a stiff mud deposited but yesterday.

At Bodö, an arctic coast station on the north side of the mouth of the Salten fjord (lat. 67° 20′), where the packets make a long halt, is a very characteristic example of this; a deposit of very tough till forming an extensive plain just on the sea level. The tide rises over this, and the waves break upon it, forming a sort of beach by washing away some of the finer material, and leaving the stones behind. The ground being so nearly level, the reach of the tide is very great, and thus a large area is exposed at low tide. Beyond the limit of high tide is an extensive inland plain covered with coarse grass and weeds growing directly upon the surface of the original flat pavement of till.

There is no river at Bodö; the sea is clear, leaves no ap-

preciable deposit, and the degree of denudation of the clayey matrix of the till is very much smaller than might be expected. The limits of high water is plainly shown by a beach of shells and stones, but at low tide the ground over which the sea has receded is a bare and scarcely modified surface of till. I have observed the same at low water at many other Arctic stations. In the Tromsö sund there are shallows at some distance from the shore which are just covered with water at low tide. I landed and waded on these, and found the bottom to consist of till covered with a thin layer of shells, odd fragments of earthenware, and other rubbish thrown overboard from vessels. It is evident that breakers of considerable magnitude are necessary for the loosening of this compact deposit, that it is very slightly, if at all, affected by the mere flow of running water.

I specify these instances as characteristic and easy of verification, as the packets all stop at these stations, but a yachtsman sailing at leisure amidst the glorious coast scenery of the Arctic Ocean might multiply such observations a hundred fold by stopping wherever such strands are indicated in passing. Of these I saw a multitude in places where I was unable to go ashore and examine them.

A further question in this direction suggested itself on the spot, viz., What is the nature of the "*banks*" which constitute the fishing grounds of Norway, Iceland, Newfoundland, &c. They are submarine plains unquestionably—they must have a high degree of fertility in order to supply food for the hundreds of millions of voracious cod-fish, coal-fish, haddocks, hallibut, &c., that people them. These large fishes all *feed on the bottom*, their chief food being mollusca and crustacea, which must find, either directly or indirectly, some pasture of vegetable origin. The banks are, in fact, great meadows or feeding grounds for the lower animals which support the higher.

From the Lofoten bank alone 20 millions of cod-fish are taken annually, besides those devoured by the vast multitude of sea birds. Now this bank is situated precisely where, according to the above-stated view of the origin of the till, there should be a huge deposit. It occupies the Vest fjord, *i.e.*, the opening between the mainland and the Lofoden Islands, extending from Moskenes, to Lodingen on Hindö, just where the culminating masses of the Kjølen Mountains must have poured their greatest glaciers into the sea by a westward course, and these glaciers must have been met by another stream pouring from the north, formed by the glaciers of Hindö, Senjenö, and both must have coalesced

with a third flood pouring through the Ofoten fjord, the Tys fjord, &c., from the mainland. The Vest fjord is about 60 miles wide at its mouth, and narrows northward till it terminates in the Ofoten fjord, which forks into several branches eastward. A glance at a good map will show that here, according to my explanation of the origin of the till, there should be the greatest of all the submarine plains of till which the ancient Scandinavian glaciers have produced, and of which the plains of till I saw on the coast at Bodö (which lies just at the mouth of the Vest fjord, where the Satten fjord flows into it), are but the slightly inclined continuation.

Some idea of this bank may be formed from the fact that outside of the Lofodens the sea is 100 to 200 fathoms in depth, that it suddenly shoals up to 16 or 20 fathoms on the east side of these rocks, and this shallow plain extends across the whole 50 or 60 miles between these islands and the mainland.* It must not be supposed the fjords or inlets of Scandinavia are *usually* shallower than the open sea; the contrary is commonly the case, especially with the narrowest and those which run farthest inland. They are *very much* deeper than the open sea.

If space permitted I could show that the great Storregen bank, opposite Aalesund and Molde, where the Stor fjord, Mold fjord, &c., were the former outlets of the glaciers from the highest of all the Scandinavian mountains, and the several banks of Finmark, &c., from which in the aggregate are taken another 20 or 30 millions of cod-fish annually, are all situated just where theoretically they ought to be found. The same is the case with the great bank of Newfoundland and the banks around Iceland, which are annually visited by large numbers of French fishermen from Dunkerque, Boulogne, and other ports.

Whenever the packet halted over these banks during our coasting trip we demonstrated their fertility by casting a line or two over the bulwark. No bait was required, merely a double

* The celebrated "maelström" is one of the currents that flow down the submarine incline between these islands when the tide is falling. Although I have ridiculed some of the accounts of this now innocent stream, I am not prepared to assert that it was always as mild as at present. If the ancient glaciers were stopped suddenly, as they may well have been, by the rocky barrier of Mosken, between Vaerö and Moskenesö, and they then suddenly concluded their deposition of till, a precipice must have been formed between this and the deep sea outside the islands, down which the sea would pitch when the tide was falling, and thus form some dangerous eddies. This cascade would gradually obliterate itself by wearing down the precipitous wall to an inclined plane such as at present exists, and down which the existing current flows.

hook with a flat shank attached to a heavy leaden plummet. The line was sunk till the lead touched the bottom, a few jerks were given, and then a tug was felt: the line hauled in with a cod-fish or hallibut hooked, not inside the mouth, but externally by the gill-plates, the back, the tail, or otherwise. The mere jerking of a hook near the bottom was sufficient to bring it in contact with some of the population. There is a very prolific bank lying between the North Cape and Nordkyn, where the Porsanger and Laxe fjord unite their openings. Here we were able, with only three lines, to cover the fore-deck of the packet with struggling victims in the course of short halts of fifteen to thirty minutes. Not having any sounding apparatus by which to fairly test the nature of the sea-bottom in these places, I cannot offer any direct proof that it was composed of till. By dropping the lead I could *feel* it sufficiently to be certain that it was not rock in any case, but a soft deposit, and the marks upon the bottom of the lead, so far as they went, afforded evidence in favour of its clayey character. A further investigation of this would be very interesting.*

But the most striking—I may say astounding—evidence of the fertility of these banks, one which appeals most powerfully to the senses, is the marvellous colony of sea-birds at Sverholtklubben, the headland between the two last-named fjords. I dare not estimate the numbers that rose from the rocks and darkened the sky when we blowed the steam-whistle in passing. I doubt whether there is any other spot in the world where an equal amount of animal life is permanently concentrated. All these feed on fish, and an examination of the map will show why—in accordance with the above speculations—they should have chosen Sverholtklubben as the best fishing ground on the arctic face of Europe.

I am fully conscious of the main difficulty that stands in the way of my explanation of the formation of the till, viz., that of finding sufficient water to float the ice, and should have given it up had I accepted Mr. Geikie's estimate of the thickness of the great ice sheet of the great ice age.

He says (page 186) that "The ice which covered the low grounds of Scotland during the early cold stages of the glacial epoch was certainly more than 2000 feet in thickness, and it must have been even deeper than this between the mainland and the Outer Hebrides. To cause such a mass to float, the

* For further particulars concerning this kind of fishing see "Through Norway with Ladies," which will be ready at about the same time as this article appears.

sea around Scotland would require to become deeper than now by 1400 or 1500 feet at least."

I am unable to understand by what means Mr. Geikie measured this depth of the ice which covered these low grounds, except by assuming that its surface was level with that of the upper ice marks of the hills beyond. The following passage on page 63 seems to indicate he really has measured it thus:—

"Now the scratches may be traced from the islands and the coast-line up to an elevation of at least 3500 feet; so that ice must have covered the country to that height at least. In the Highlands the tide of ice streamed out from the central elevations down all the main straths and glens; and by measuring the height attained by the smoothed and rounded rocks we are enabled to estimate roughly the probable thickness of the old ice-sheet. But it can only be a rough estimate, for so long a time has elapsed since the ice disappeared, the rain and frost together have so split up and worn down the rocks of these highland mountains, that much of the smoothing and polishing has vanished. But although the finer marks of the ice chisel have thus frequently been obliterated, yet the broader effects remain conspicuous enough. From an extensive examination of these we gather that the ice could not have been less, and was probably more, than 3000 feet thick in its deepest parts."

Page 80 he says—"Bearing in mind the vast thickness reached by the Scotch ice-sheet, it becomes very evident that the ice would flow along the bottom of the sea with as much ease as it poured across the land, and every island would be surmounted and crushed, and scored and polished just as readily as the hills of the mainland were."

Mr. Geikie describes the Scandinavian ice-sheet in similar terms, but ascribes to it a still greater thickness. He says (page 404)—"The whole country has been moulded and rubbed and polished by an immense sheet of ice, which could hardly have been less than 6000 or even 7000 feet thick," and he maintains that this spread over the sea and coalesced with the ice-sheet of Scotland.

My recollection of the Lofoden Islands, which from their position afford an excellent crucial test of this question, led me to believe that their configuration presented a direct refutation of Mr. Geikie's remarkable inference; but a mere recollection of scenery being too vague, a second visit was especially desirable in reference to this point. The result of the special observations I made during this second visit fully confirmed the impression derived from memory.

I found in the first place that all along the coast from Stavanger to the Varanger fjord every rock *near the shore* is glaciated; among the thousands of low-lying ridges that peer above the water to various heights none near the mainland are angular. The general character of these is shown in the sketch of "My Sea Serpent," in the last edition of "Through Norway with a Knapsack."

The rocks which constitute the extreme outlying limits of the Lofoden group, and which are between 60 and 70 miles from the shore, although mineralogically corresponding with those near the shore, are totally different in their conformation, as the sketch of three characteristic specimens plainly shows. Mr. Everest very aptly compares them to shark's teeth. Proceeding northward, these rocks gradually progress in magnitude, until they become mountains of 3000 to 4000 feet in height; their outspread bases form large islands, and the west fjord gradually narrows.

The remarkably angular and jagged character of these rocks when weathered in the air renders it very easy to trace the limits of glaciation on viewing them at a distance. The outermost and smallest rocks show from a distance no signs of glaciation. If submerged, the ice of the great ice age must have floated over them; if above the sea as at present, they can have suffered no more glaciation than would be produced by such an ice-sheet as that of the "paleocrystic" ice recently found by Capt. Nares on the North of Greenland. Progressing northward, the glaciation begins to become visible, running first up to about 100 feet above the sea-level on the islands lying westward and southward of Ost Vaagen. Further northward along the coast of Ost Vaagen and Hindö, the level gradually rises to about 500 feet on the northern portion of Ost Vaagen, and up to more than 1000 feet on Hindö, while on the mainland it reaches 3000 to 4000 feet.

A remarkable case of such variation, or descent of ice-level as the ice-sheet proceeded seaward is shown at Tromsö. This small oblong island (lat. 69° 40′), on which is the capital town of Finmark, lies between the mainland and the large mountainous island of Kvalö, with a long sea-channel on each side, the Tromösund and the Sandesund; the total width of these two channels and the island itself being about 4 or 5 miles. The general line of glaciation from the mainland crosses the channels and the island, which has evidently been buried and ground down to its present moderate height of two or three hundred feet. Both of the channels are till-paved. On the east or inland side the mountains near

the coast are glaciated to their summits—are simply *roches-montonnes* over which the reindeer of the Tromsdal Lapps range and feed. On the west the mountains are dark pyramidal peaks, with long vertical snow streaks marking their angular masses.

The contrast is very striking when seen from the highest part of the island, and is clearly due to a decline in the thickness of the ice-sheet in the course of its journey across this narrow channel. Speaking roughly from eye estimation, I should say that this thinning or lowering of the limits of glaciation exceeds 500 feet between the opposite sides of the channel, which, allowing for the hill slopes, is a distance of about 6 miles. This very small inclination would bring a glacier of 3000 feet in thickness on the shore down to the sea-level in an outward course of 30 miles, or about half the distance between the mainland and the outer rocks of the Lofodens.

I am quite at a loss to understand the reasoning upon which Mr. Geikie bases his firm conviction respecting the depth of the ice sheet on the low grounds of Scotland and Scandinavia. He seems to assume that the glaciers of the great ice age had little or no superficial down slope corresponding to the inclination of the base on which they rested. I have considerable hesitation in attributing this assumption to Mr. Geikie, and would rather suppose that I have misunderstood him, as it is a conclusion so completely refuted by all we know of glacier phenomena and the physical laws concerned in their production, but the passages I have quoted and several others are explicit and decided.

Those geologists who contend for the former existence of a great polar ice-cap radiating outwards and spreading into the temperate zones might adopt this mode of measuring its thickness, but Mr. Geikie rejects this hypothesis, and shows by his map of "The Principal Lines of Glacial Erosion in Sweden, Norway, and Finland," that the glaciation of the extreme north of Europe proceeded from south to north; that the ice was formed on land, and proceeded seawards in all directions.

I may add to this testimony that presented by the North Cape, Sverholt, Nordkyn, and the rest of the magnificent precipitous headlands that constitute the characteristic feature of the arctic face of Europe. They stand forth defiantly as a phalanx of giant heralds proclaiming aloud the fallacy of this idea of southward glacial radiation; and in concurrence with the structure and striation of the great glacier troughs that lie between them, and the planed table-land at

their summits, they establish the fact that during the greatest glaciation of the glacial epoch the ice-streams were formed on land and flowed out to sea, just as they now do at Greenland, or other parts of the world where the snow-line touches or nearly approaches the level of the sea.

All such streams must have followed the slope of the hill-sides upon which they rested and down which they flowed, and thus the upper limits of glaciation afford no measure whatever of the thickness of the ice upon "the low grounds of Scotland," or of any other glaciated country. As an example I may refer to Mont Blanc. In climbing this mountain the journey from the lower ice-wall of the Glacier de Bossons up to the *bergschrund* above the *Grand plateau* is over one continuous ice-field, the level of the upper part of which is about 10,000 feet above its terminal ice-wall. Thus, if we take the height of the striations or smoothings of the upper *nevé*, above the low grounds on which the ice-sheet rests, and adopt Mr. Geikie's reasoning, the lower ice-wall of the Glacier de Bossons should be 10,000 feet thick. Its actual thickness, as nearly as I can remember, is about 10 or 12 feet.

Every other known glacier presents the same testimony. The drawing of a Greenland glacier opposite page 47 of Mr. Geikie's book shows the same under arctic conditions, and where the ice-wall terminates in the sea.

I have not visited the Hebrides, but the curious analogy of their position to that of the Lofodens suggests the desirability of similar observations to those I have made in the latter. If the ice between the mainland and the Outer Hebrides was, as Mr. Geikie maintains, "certainly more than 2000 feet in thickness," and this stretched across to Ireland, besides uniting with the still thicker ice-sheet of Scandinavia, these islands should all be glaciated, especially the smaller rocks. If I am right the smaller outlying islands, those south of Barra, should, like the corresponding rocks of the Lofodens, display no evidence of having been overswept by a deep "*mer de glace*."

I admit the probability of an ice-sheet extending as Mr. Geikie describes, but maintain that it thinned out rapidly seaward, and there became a mere ice-floe, such as now impedes the navigation of Smith's Sound and other portions of the Arctic Ocean. The Orkneys and Shetland, with which I am also unacquainted, must afford similar crucial instances, always taking into account the fact that the larger islands may have been independently glaciated by the accumulations due to their own glacial resources. It is the small rocks standing at considerable distance from the shores

of larger masses of land that supply the required test-conditions.

From the above it will be seen that I agree with Mr. Geikie in regarding the till as a "*moraine profonde*," but differ as to the mode and place of its deposition. He argues that it was formed under glaciers of the thickness he describes, while their whole weight rested upon it.

This appears to me to be physically impossible. If such glaciers are capable of eroding solid rocks, the slimy mud of their own deposits could not possibly have resisted them. The only case where this might have happened is where a mountain-wall has blocked the further downward progress of a glacier, or in pockets or steep hollows which a glacier might have bridged over and filled up; but such pockets are by no means the characteristic localities of till, though the till of Switzerland may possibly show examples of the first case. The great depth of the inland lakes of Norway, their bottoms being usually far below that of the present sea-bottom, is in direct contradiction of this.* They should, before all places, be filled with till, if the till were a ground moraine formed on land; but all we know of them confirms the belief that the glaciers deepened them by erosion instead of shallowing them by deposition.

Mr. Geikie's able defence of Ramsay's theory of lake basin erosion is curiously inconsistent with his arguments in favour of the ground moraine.

I fully concur with Mr. Geikie's arguments against the iceberg theory of the formation of the till. This I think he has completely refuted.

Before concluding I must say a few words on those curious lenticular beds of sand and gravel in the till which appear so very puzzling. A simple explanation is suggested in connection with the above-sketched view of the formation of the till. All glaciers, whether in arctic or temperate climates, are washed by streamlets during summer, and these commonly terminate in the form of a stream or cascade pouring down a "*moulin*," or well bored by themselves and reaching the bottom of the glacier. Now what must be the action of such a downflow by water upon my supposed submarine bed

* The largest of the Norwegian lakes, the Mjosen, is 1550 feet deep, and its surface 385 feet above the sea level. Its bottom is about 1000 feet lower than the sea outside, or 500 to 800 feet below the bottom of the Christania Fjord. The fjords, generally speaking, are very much shallower near their mouths than further inland, as though their depth had been determined by the thickness of the glaciers flowing down them, and the consequent limits of flotation and deposition.

of till just grazing the bottom of the glacier? Obviously to wash away the fine clayey particles, and leave behind the coarser sand or gravel. It must form just such a basin or lenticular cavity as Mr. Geikie describes. The oblong shape of these, their longer axis coinciding with the general course of the glacier, would be produced by the onward progress of the moulin. The accordance of their other features with this explanation will be seen on reading Mr. Geikie's description (pp. 18, 19, &c.).

The general absence of marine animals and their occasional exceptional occurrence in the intercalated beds is just what might be expected under the conditions I have sketched. In the gloomy subglacial depths of the sea, drenched with continual supplies of fresh water and cooled below the freezing-point by the action of salt water on the ice, ordinary marine life would be impossible; while, on the other hand, any recession of the glacial limit would restore the conditions of arctic animal life, to be again obliterated with the renewed outward growth of the floating skirts of the inland ice mantle.

But I must now refrain from the further discussion of these and other collateral details, but hope to return to them in another paper.

In "Through Norway with Ladies" I have touched lightly upon some of these, and have more particularly described some curious and very extensive evidences of secondary glaciation that quite escaped my attention on my first visit, and which, too, have been equally overlooked by other observers. In the above I have endeavoured to keep as nearly as possible to the main subject of the origin of the till and the character of the ancient ice-sheet.

VI. THE UNITED STATES GEOLOGICAL AND GEOGRAPHICAL SURVEY OF THE TERRITORIES.—EXPLORATIONS IN 1876.

By Prof. F. V. HAYDEN, Geologist in Charge.

THE various parties composing the United States Geological and Geographical Survey of the Territories commenced their field-work in August. Owing to the evidences of hostility among the northern tribes of Indians it was deemed most prudent to confine the labours of the Survey to the completion of the Atlas of Colorado. Therefore the work of the season of 1876 was a continuation of the labours of the three preceding years, westward, finishing the entire mountainous portion of Colorado, with a belt of 15 miles in width of northern New Mexico, and a belt 25 miles in breadth of eastern Utah. Six sheets of the Physical Atlas are now nearly ready to be issued from the press. Each sheet embraces an area of over 11,500 square miles, or a total of 70,000 square miles. The maps are constructed on a scale of 4 miles to 1 inch, with contours of 200 feet, which will form the basis on which will be represented the geology, mines, grass, and timber lands, and all lands that can be rendered available for agriculture by irrigation. The areas of exploration the past season are located in the interior of the continent, far remote from settlements, and among the hostile bands of Ute Indians that attacked two of the parties the previous year.

The point of departure the past season was Cheyenne, Wyoming Territory. Two of the parties, with all their outfit, were transported by railroad to Rawlins Springs, and proceeded thence southward. The other two were sent by railroad from Cheyenne southward, one party to Trinidad and the other to Cañon City.

The primary triangulation party was placed in charge of A. D. Wilson, and took the field from Trinidad, the southern terminus of the Denver and Rio Grande Railroad, August 18th, making the first station on Fisher's Peak. From this point the party marched by the valley of the Purgatoire,

crossed the Sangre de Cristo range by way of Costilla Pass, followed the west base of the range northward as far as Fort Garland, making a station on Culebra Peak.

About 6 miles north of Fort Garland is located one of the highest and most rugged mountain peaks in the west, called Blanca Peak, the principal summit of the Sierra Blanca group. On the morning of August 28th the party, with a pack-mule to transport the large theodolite, followed up a long spur which juts out to the south. They found no difficulty in riding to timber line, which is here about 12,000 feet above sea level. At this point they were compelled to leave the animals, and, distributing the instruments among the different members of the party, proceeded on foot up the loose, rocky slope to the first outstanding point, from which a view could be obtained of the main peak of the range. Although this first point is only 600 feet lower than the main summit, yet the most arduous portion of the task was to come. The main summit is about 2 miles north of the first point, in a straight line, and connected with it by a very sharp-toothed, zigzag ridge, over which it is most difficult to travel, on account of the very loose rocks and the constant fear of being precipitated down on either side several hundred feet into the amphitheatres below. After some two hours of this difficult climbing they came to the base of the main point, which, though very steep, was soon ascended, and at 11 o'clock A.M. they found themselves on the very summit. From this point one of the most magnificent views in all Colorado was spread out before them. The greater portion of Colorado and New Mexico was embraced in this field of vision. This point is the highest in the Sierra Blanca group, and, so far as is known at the present time, is the highest in Colorado. The elevation of this point was determined by Mr. Wilson in the following manner :—First, by a mean of eight barometric readings, taken synchronously with those at Fort Garland, which gave a difference between the two points of 6466 feet ; secondly, by fore and back angles of elevation and depression, which gave a difference of 6468 feet. The elevation at the fort was determined by a series of barometric readings, which, when compared with the Signal Service barometer at Colorado Springs, gave it an elevation of 7997 feet, making the Blanca Peak 14,464 feet above the sea level. This peak may be regarded, therefore, as the highest, or at least next to the highest, yet known in the United States. A comparison with some of the first-class peaks in Colorado will show the relative height :—

			Feet.
Uncompahgre Peak,	above sea level	. .	14,235
Blanca Peak,	,, ,,	. .	14,464
Mount Harvard,	,, ,,	. .	14,384
Gray's Peak,	,, ,,	. .	14,341
Mount Lincoln,	,, ,,	. .	14,296
Mount Wilson,	,, ,,	. .	14,280
Long's Peak,	,, ,,	. .	14,271
Pike's Peak	,, ,,	. .	14,146

The foregoing table will afford some conception of the difficulty encountered in determining the highest peak where there are so many that are nearly of the same elevation. About fifty peaks are found within the limits of Colorado that exceed 14,000 feet above the sea level.

From this point the party proceeded westward across the San Luis Valley, and up the Rio Grand to its source, making two primary stations on the way, one near the summit district and the other on the Rio Grande pyramid. From the head of the Rio Grande the party crossed the continental divide, striking the Animas Park, and thence proceeded by trail to Parrott City.

After making a station on La Plata Peak the party marched north-west across the broken mesa country west of the Dolores, making three stations on the route to complete a small piece of topography that had been omitted the previous year, on account of the hostility of the Ute Indians. After making a primary station on the highest point of the Abajo mountains, the party turned eastward to Lone Cone, where another station was made. Thence crossing the Gunnison and Grand Rivers, they proceeded to the great volcanic plateau at the head of White River. The final station was made between the White and Yampah Rivers, in the north-western corner of Colorado. During this brief season Mr. Wilson finished about 1000 square miles of topography, and made eleven primary geodetic stations, thus connecting together by a system of primary triangles the whole of Southern and Western Colorado.

In company with the triangulation party, Mr. Holmes made a hurried trip through Colorado, touching, also, portions of New Mexico and Utah. He was unable to pay much attention to detailed work, but had an excellent opportunity of taking a general view of the two great plain-belts that lie the one along the east, the other along the west base of the Rocky Mountains. For nearly 2000 miles' travel he had constantly in view the cretaceous and tertiary formations among which are involved some of the most interesting

geological questions. He observed, among other things, the great persistency of the various groups of rocks throughout the east, west, and north, and especially in the west; that from northern New Mexico to south-west Wyoming, the various members of the cretaceous lie in almost unbroken belts.

Between the east and west there is only one great incongruity. Along the east base of the mountains the upper cretaceous rocks, including Nos. 4 and 5, are almost wanting, consisting at most of a few hundred feet of shales and laminated sandstones. Along the west base this group becomes a prominent and important topographical as well as geological feature. In the south-west, where it forms the "Mesa Verde" and the cap of the Dolores plateau, it comprises upward of 2000 feet of coal-bearing strata, chiefly sandstone, while in the north it reaches a thickness of 3500 feet, and forms the gigantic "hog-back" of the Grand River Valley.

While in the south-west he visited the Sierra Abajo, a small group of mountains which lie in eastern Utah, and found, as he had previously surmised, that the structure was identical with that of the four other isolated groups that lie in the same region. A mass of trachyte has been forced up through fissures in the sedimentary rocks, and now rests chiefly upon the sandstones and shales of the lower cretaceous. There is a considerable amount of arching of the sedimentary rocks, caused probably by the intrusion of wedge-like sheets of trachyte, while the broken edges of the beds are frequently but abruptly up, as if by the upward or lateral pressure of the rising mass. He was able to make many additional observations on the geology of the San Juan region, and secured much valuable material for the colouring of the final map.

He states that the northern limit of ancient cliff-builders in Colorado and Eastern Utah is hardly above latitude 37° 45′.

The Grand River division was directed by Henry Gannett, topographer, with Dr. A. C. Peale as geologist. James Stevenson, executive officer of the Survey, accompanied this division, for the purpose of assisting in the management of the Indians, who last year prevented the completion of the work in their locality by their hostility.

The work assigned this division consisted in part of a small area, containing about 1000 square miles, lying south of the Sierra la Sal. The greater portion of the work of this division lay north of the Grand River, limited on the

north by the parallel of 39° 30′, and included between the meridian of 108° and 109° 30′.

This division took the field at Canon City, Colorado, about the middle of August. The party travelled nearly west up the Arkansas River, over Marshall's Pass, and down the Tomichi and Gunnison Rivers to the Uncompahgra (Ute) Indian agency. Here they secured the services of several Indians as escort to the somewhat dangerous country which they were first to survey. This area, lying south of Sierra la Sal, was worked without difficulty. It is a broken plateau country, and presents many extremely curious pieces of topography. Eleven days were occupied in this work.

The Grand River, from the mouth of the Gunnison River to that of the Dolores, *i.e.*, for nearly a hundred miles, flows along the southern edge of a broad valley, much of the way being in a low canon, 100 to 200 feet deep. The course of the river is first north-west for 25 miles; then turning abruptly, it flows south-west, and then south, for about 75 miles. This valley has an average width of 12 miles. It is limited on the north and west by the "Roan or Book Cliffs," and their foot-hills, which follow the general course of the river. These cliffs rise from the valley in a succession of steps to a height of about 4000 feet above it, or 8000 to 8500 feet above the sea.

From its crest this plateau (for the Book Cliffs are but the southern escarpment of a plateau) slopes to the N.N.E. at an angle of not more than five degrees. It extends from the Wahsatch mountains, on the west, to the foot hills of the Park range, on the east, and presents everywhere the same characteristics. The Green River crosses it, flowing in a direction exactly the reverse of the dip. It borders the Grand on the north for 100 miles, the crest forming the divide between the Grand and the White. On the south side of the crest are broken cliffs; on the north side, the branches of the White Canon immediately. This leaves the divide in many places very narrow, in some cases not more than 30 to 40 feet wide, with a vertical descent on the south toward the Grand River, and an extremely steep earth-slope (35 degrees in many cases) at the heads of the streams flowing north to the White River. This crest, though not over 8500 feet in height, is the highest land for a long distance in every direction.

After leaving the Uncompahgre agency, the party followed Gunnison's Salt Lake road to the Grand, and down that river to the mouth of the Dolores, in latitude 38° 50′, longitude 109° 17′. At this point they turned northward,

and went up to the crest of the Book plateau. They followed the crest to the eastward for upward of a hundred miles, or to longitude 108° 15′; then descended to the Grand, and followed it up to longitude 107° 35′, and thence *via* the White River (Ute) Indian agency, to Rawlins, where they arrived on October 23rd.

The whole area worked is about 3500 square miles, in surveying which about sixty stations were made.

The geological work of this division, by Dr. Peale, connects directly with that done by him in 1874 and 1875. Sedimentary formations prevail on both districts visited during the past season.

The country first examined lies between the San Miguel and Dolores Rivers, extending northward and north-westward from Lone Cone Mountain. The general character of this region is that of a plateau cut by deep gorges or canons, some of which, especially toward the north, extend from the sandstones of the Dakota group to the top of the Red Beds. The depth of the canon, however, is no indication of its importance as a stream-bed, for, excepting the main streams, they are dry the greater portion of the year. There are not great disturbances of the strata, what folds do occur being broad and comparatively gentle.

The San Miguel River, on leaving the San Juan mountains, flows toward the north-west, and, with its tributaries, cuts through the sandstones of the Dakota group, exposing the variegated beds lying beneath, that have generally been referred to the Jurassic. About 25 or 30 miles north of Lone Cone, the river turns abruptly to the west, and flows west and south-west for about 15 miles, when it again turns and flows generally north-west, until it joins the Dolores. Between the San Miguel and Lone Cone the sandstones of the Dakota group, or No. 1 cretaceous, are nearly horizontal, forming a plateau which, on approaching the mountains, has a copping of cretaceous shales.

Beyond the bend the San Miguel flows in a monoclinal valley, in which the canon walls are of the same description as in the upper part of its course. As the mouth is approached the Red Beds appear. Between this portion of the course of the San Miguel and the almost parallel course of the Dolores, which is in a similar monoclinal rift, there are two anticlinal and two synclinal valleys parallel to each other. They are all occupied by branches of the Dolores lower cretaceous, jurassic, and triassic strata outcrop, and present some interesting geological details, which will be fully considered in the report on the District. The Dolores

River comes from a high plateau in a zigzag course, flowing sometimes with the strike, and sometimes with the dip of the strata. Its general course on the western line is about north-west, from which it turns to the northward and westward, finally changing to north-west again, to its junction with the Grand. It is in canon the greater part of its course.

In the region of country north of Grand River the geological formations extend uninterruptedly from the Red Beds exposed on Grand River to the white tertiary cliffs forming the summit of the "Roan Mountains," or Book Cliffs. The Grand is generally in a canon in the Red Beds; on the north side the No. 1 cretaceous sandstone forms a hog-back, sloping towards the cliffs. Between the crest of this hog-back and the cliffs there is a broad valley formed by the erosion of the soft cretaceous shales which extend to the base of the cliffs, and in some places form their lower portion. The cliffs are composed mainly of cretaceous beds, rising one above another in steps until an elevation of about 8000 feet is reached. The summit is the edge of a plateau sloping to N.N.E. This plateau is cut by the drainage flowing into the White River from the south. These streams rarely cut through the tertiary series.

Coal of poor quality is found in the sandstones of the Dakota group, and also in the sandstones above the middle cretaceous beds. Wherever noticed it was in their seams, and of little economic importance.

The White River division was directed by G. B. Chittenden, as topographer, accompanied by F. M. Endlich, as geologist.

The district assigned to this party as their field for exploration during the season of 1876 commenced from the eastward at longitude 107° 30′, joining on to the work previously done, and extended westward 30 miles into Utah Territory. Its southern boundary was N. latitude 39° 38′, while the White River formed the northern limit. In order to complete to the greatest possible advantage in the short time that could be allowed, it was determined to make the White River Agency head-quarters, and in two trips from there complete the work. About 3800 square miles comprised the area surveyed.

In working up the topography of this district the party spent forty-eight days of absolute field-work, made forty-one main topographical stations and sixteen auxiliary ones, and travelled within the district about 1000 miles. The party ascertained the courses of all the main trails, the location

and quality of almost all the water, which is scanty throughout, and can map with considerable accuracy the topographical forms and all the water-courses. The area is almost entirely devoid of topographical "points," and the topographer is obliged to depend to a considerable degree on those far to the north and south for the triangulation. The country has heretofore been almost entirely unexplored, and was described by the nearest settlers as a broken canon country, extremely dry. It was marked on the maps as a high, undulating plateau, with fresh-water lakes and timber. The party saw no lakes of more than 400 yards in diameter, and only two or three of these. The country is nearly all inhabitable, both winter and summer, and considerable portions of it valuable; and though three-quarters of it is within the Ute Indian reservation, the advantage of a more accurate knowledge of its character can readily be seen.

While working in the low broken country of south-western Colorado last year, Mr. Chittenden made use of a light portable plane-table, and found it of great value. It appeared at that time that its value was greatest in that class of country, and that in a low rolling district, with few prominent points, or in a high mountain country, it would probably be of little or no use. Altitudes were determined by the mercurial barometer, with a base at the White River Indian Agency, and checked by a continuous system of vertical angles. The altitude of the agency has been determined by a series of barometric observations extending over two years and a half, and referred to railroad levels, and can probably be depended on to within a few feet. The altitude of the agency being about 6500 feet, and the altitudes in the district ranging from 5000 to 8000 feet, makes its location the best possible in height for a barometric survey of the region.

It is the intention of the Survey, during the coming year, to publish some tabulated results of the barometric work in Colorado, showing the system, and its accuracy and reliability. This may be of use in future work, since the topography of the whole west must greatly depend on barometric determinations of altitude, and Colorado has furnished almost every possible phase of western topography.

The longest dimension of the work lying east and west, and the White and Grand Rivers running in approximately parallel courses, the district stretched from the White River up over the divide between the Grand and White, and embraced the heads of the lateral drainage of the former river.

The general topography is a gentle rise from the White River toward the south, and a sudden breaking off when

the divide is reached into rugged and often impassable cliffs, known on the maps as the Roan or Book Mountains. The gentle plateau slope of the White River side is cut by almost numberless and often deep canons, and in many cases the surface of the country has been eroded away, leaving broken and most picturesque forms, the lower benches generally covered with cedars and piñons, and the upper rich in grass.

There are four main streams draining into the White River within the limits of our work—a distance of something over 100 miles. The easternmost is a large running stream; the second, though tolerably good water may be found in pools in its bed, carries in the summer no running water for the greater part of its course; the third has for most of its length a trickling stream of the bitterest of alkali water, while the fourth and westernmost one is perfectly dry for some 25 miles from its mouth, and then forks, one branch containing pure sweet water in pools, the other a running stream of bitter alkali. All of these streams have more or less good water at their heads. They travelled nearly the whole length of all these water-courses, but found good trails only in the two middle ones. Trails, which traverse the whole district in every possible direction, keep mostly on the summits of the ridges and plateaus, and by taking care not to cross the canons, the country is very easily travelled through.

The country is almost entirely destitute of timber, and has but little good water. It is, however, abundantly, richly supplied with grass, and, especially in the winter season, must be well stocked with game. It seems well adapted to its present use as an Indian reservation, and is likely to remain for years to come more valuable for them than it could be for settlement.

In the far western portion, and outside the limits of the reservation, one large vein of asphaltum and several small veins were found, and also running springs of the same material, all of which, if once reached by railroads, will prove of great commercial value. These deposits have been spoken of before, but their location has not been accurately determined. The principal vein seen by this party is at present about 100 miles from railroad communication, but less than half that distance from white settlement, and is likely in the present rapid growth of that country to be within a few years made available.

According to the report of F. M. Endlich, the geology of this district is very simple, though interesting. Inasmuch

as but one divide of importance occurred within the district, the work was somewhat simplified. This was formed by the Book Cliffs, between the drainages of the Grand on the south and the White on the north. Both these rivers flow a little south of west, into Green River, which they join in Utah. From the junction of the Grand and Green downward the river is called the Great Colorado. Orograpically, the region surveyed is comparatively simple. The Book Cliffs are the summit of a plateau about 8000 feet above sea-level, continuing unbroken over to the Green River. Toward the south these cliffs fall off very steeply, forming deep canons that contain tributaries of the Grand River. On the north side, with the dip of the strata, the slope is more gentle, although, in consequence of erosion, numerous precipitous cliffs are found. Descending in that direction, the character of the country changes. Instead of an unbroken slope, we find that the plateau has been cut parallel by the White River drainage, and the long characteristic mesas of that region testify to the action of erosion. Approaching the river, constantly descending with the slight dip of the strata, the bluffs become lower and lower. Though the creek-valleys are wide, and at certain seasons no doubt well watered, the vegetation is that of an arid country. Dwarf pines, piñons, and sagebrush abound, to the almost entire exclusion of other trees or grass. Travelling down White River, this character is again found to change. A new series of bluffs, occasioned by heavy superincumbent strata, gives rise to the formation of deep canons. For 45 miles the party followed the canon of the White; that, no doubt, is analogous to that of the Green, and probably closely resembles that of the Colorado in its detail features. Vertical walls enclose the narrow river-bottoms, and the slopes of the higher portions are ornamented by thousands of curiously eroded rocks. "Monuments" of all kinds, and figures that can readily be compared to those of animated beings, enliven the scenery, which otherwise would be very monotonous; 2000 to 3000 feet may be stated as the height of the walls enclosing the White River.

Geologically speaking, the district was one of singular uniformity. Travelling westward, the older formations, reaching back as far as the triassic, were found. This was followed by cretaceous, which in turn was covered by tertiary. About three-quarters of the region surveyed was found to contain beds belonging to this period. Owing to the lithological character of the strata water was a rare luxury in this region, and men and animals were frequently

dependent upon looking for springs. Farther west still the Green River group sets in, forming those numerous canons of which that of the White River is one.

Having completed their work by October 14th, the party marched eastward through Middle Park, and after twelve days of rain and snow reached Boulder City, Colorado.

The field work of the Yampah division during the past season was principally confined to a district of north-eastern Colorado lying between the Yampah and White Rivers, and between Green River and the subordinate range of mountains that lies west of and parallel with the Park range. The area is embraced between parallels 39° 30′ and 40° 30′, and meridian 107° 30′ and 109° 30′.

The party consisted of Mr. G. R. Bechler, topographer, directing, accompanied by Dr. C. A. White, the well known geologist. They proceeded southward from Rawlins Springs, a station on the Union Pacific Railroad, August 6th, toward their field of labour. From Rawlins Springs to Snake River, a distance of 80 miles, table lands form the chief feature of the topography, while from Snake River to the Yampah River the surface is more undulating, and thickly covered with sage. Between the Yampah and White Rivers, a distance of 50 miles, the country is mountainous, and on the divide between the Yampah and White Rivers the elevation is 8000 to 9000 feet. Mr. Bechler, after having formed the geodetic connection with the work of previous years, concluded to finish the more mountainous portion of the area assigned to him, which began from a line of meridian with the White River Agency, and extended westward to about 108° 10′. Here the party found water and grass in abundance, with one exception. The plateau country, however, was so destitute of water, and so cut up with dry gorges or canons, with scarcely any grass or timber of any kind, that travelling was rendered very difficult. The party therefore made White River its base of supply for water and grass, making side trips into the barren hill-tops or plateaus in every direction.

From the Ute Agency, which is located approximately in latitude 38° 58′ and longitude 107° 48′, the White River takes an almost due west course for 15 or 18 miles, most of the way through an open valley, with here and there narrow gorges. About 50 miles from the Agency the river opens into a broad barren valley, with only here and there scanty patches of vegetation. Soon after, the river enters a deep canon, with vertical walls 1000 feet or more in height, and continues to increase in depth until the river flows into the Colorado of the west.

The Yampah, or Bear River, deviates from a westerly course only for a few miles occasionally. Like White River it flows through a plateau country, which rises gently from the river, back for a distance of about eight miles. South of the river lie the Williams River Mountains, which have a gradual slope to the north. Williams fork, flowing from a south-eastern direction, joins the Yampah river west of the junction. The Yampah traverses the country more or less in a canon, occasionally emerging into an open grassy valley, then enters a deep canon, cuts through the Yampah Mountains, when it joins with the Snake River. The place of junction resembles a fine park, surrounded on all sides with eroded terraces and plateau spurs that rise by steps to the divide on either side. This park is about 8 miles in length from east to west. After leaving this park the river enters a hugh fissure in the mountains, where it remains, until, completing its zigzag course, it joins the Green River in longitude 109° 40′, and latitude 32°. After the junction with the Yampah, the Green River continues in a canon for 14 miles, where it passes through the picturesque palisades of Split Mountain into an open broad valley, longitude 109° 15′, latitude 40° 28′, from which point it takes a south-west direction through the Wamsitta Valley, where it unites with the White River. Into both White and Yampah Rivers numerous branches extend from either side, forming deep canons the greater portion of their length. We may say, in brief, that the sides of the valleys expand and contract, at one time forming the beautiful grassy valleys which in olden times were celebrated as the favourite wintering places for the trappers, or contracting so as to form narrow canons or gorges with walls of varied height.

The walls of Yampah canon average about 1000 feet, while the mountains, receding back to the northward, attain an elevation of 4200 feet, while the highest point of the plateau on the south side is 3400 feet above the river-level.

Of the plateaus between White and Yampah Rivers, Yampah plateau is the largest, and occupies an area of 400 square miles. The surface of the summit is undulating, and on the south side it presents a steep face, several hundred feet in height, covered with *débris*, rendering it almost inaccessible. This plateau is covered with excellent grass, and gives origin to numerous springs, all of which dry up within a short distance of their source.

As a whole, this district is very arid, barren, and almost destitute of tree vegetation.

The total number of stations made by Mr. Bechler in the

district assigned to him was 40, and the entire area was about 3000 square miles. Barometric observations were made whenever needed, and about 2000 angles of elevation and depression with fore- and back-sights, so that material for obtaining correct altitudes is abundant.

The rocks of this district embrace all the sedimentary formations yet recognised by the investigators who have studied the region that lies between the Park Range and the Great Salt lake, namely, from the Uinta quartzite (which underlies the carboniferous) to the Brown's Park group, or latest tertiary inclusive. Not only has the geographical distribution of these formations been mapped, but all the displacements of the strata have been traced and delineated. The last-named investigations bring out some interesting and important facts in relation to the orthographic geology of the region, especially as regards the eastern termination of the great Uinta uplift, and the blending of its vanishing primary and accessory displacements with those of the north and south range above mentioned. Much information was also obtained concerning the distribution of the local drift of that region, the extent and geological date of outflow of trap, &c.

The brackish water-beds at the base of the tertiary series, containing the characteristic fossils, were discovered in the valley of the Yampah. They are thus shown to be exactly equivalent with those, now so well known, in the valley of Bitter Creek, Wyoming Territory. These last-named localities were also visited at the close of the season's work, and from the strata of this horizon at Black Buttes Station three new species of *Unio* were obtained, making six clearly distinct species in all that have been obtained, associated together in one stratum at that locality. They are all of either distinctively American types or closely related to species now living in American fresh waters. They represent by their affinities the following living species:—*Unio clavus*, Lamarck: *U. securis*, Lea; *U. gibbosus*, Barnes; *U. metaneorus*, Rafinesque; and *U. complanatus*, Solander. They are associated in the same stratum with species of the genera *Corbulo*, *Corbicula*, *Neritina*, *Viriparus*, &c., and which stratum alternates with layers containing *Ostrea* and *Anomia*.

The close affinity of these fossil *Unios* with species now living in the Mississippi River and its tributaries seems plainly suggestive of the fact that they represent the ancestry of the living ones. An interesting series of facts has also been collected, showing that some of the so-called American types of *Unio* were introduced in what is now the

great Rocky Mountain region as early as the Jurassic period, and that their differentiation had become great and clearly defined as early as late cretaceous and early tertiary times. Other observations suggest the probable lines of geographical distribution, during the late geological periods of their evolutional descent, by one or more of which they have probably reached the Mississippi River system, and culminated in the numerous and diverse forms that now exist there.

The work of the past season shows very clearly the harmonious relations of the various groups of strata over vast areas; that although there may be a thickening or a thinning out of beds at different points, they can all be correlated from the Missouri River to the Sierra Nevada Basin. The fact also that there is no physical or palæontological break in these groups over large areas from the cretaceous to the middle tertiary is fully established. The transition from marine to brackish water forms of life commences at the close of the cretaceous epoch, and without any line of separation that can yet be detected, continues on upward until only purely fresh-water forms are to be found. Dr. White, an eminent palæontologist and geologist, says that the line must be drawn somewhere between the cretaceous and tertiary epochs, but that it will be strictly arbitrary, as there is no well-marked physical break to the summit of the Bridger group.

NOTICES OF BOOKS.

The Primæval World of Switzerland. With 560 Illustrations. By Prof. Heer, of the University of Zurich. Edited by James Heywood, M.A., F.R.S. London: Longmans and Co.

We have here, under an unpretending title, a work so full of interest that the critic must pause, uncertain to what portions he ought principally to direct attention. The ordinary traveller who "does" Switzerland in the orthodox fashion, Murray in hand, or even the enthusiastic worshipper of Alpine scenery, has little conception of the wonders that lie beneath his feet. But to the enquiring mind of Prof. Heer the rocks have rendered up their secrets, and no portion of the world has been found more fruitful in evidence on the climatology and the geography, physical and organic, of bye-gone periods. The author before the appearance of this work was already honourably known from his "Fossil Fauna of the Arctic Regions," his "Swiss Tertiary Flora," and his "Plants of the Swiss Lake Dwellings." But in the volumes now before us he embodies the whole of his researches on the past development of his native country, and enables the student to examine the succession of plants and animals in the different geological formations, and thus to obtain information on weighty biological questions. As a matter of course the author belongs to the honourable class of monographists—observers who select some well-defined task and strive to master it fundamentally. But at the same time, whilst examining the Swiss fossilised annals of natural history, he does not neglect the light, complementary, explanatory, or corrective, which may be drawn from the changes of other parts of the globe.

The mere economical geologist, whose attention is exclusively confined to the occurrence of metallic ores and other marketable minerals, will not find in these volumes much to enchain his attention. Switzerland is not a mining country. True coal, with the exception of the anthracite beds of the Valais, can scarcely be said to exist. An argentiferous copper ore occurs in the Sernft Rock, and was worked on the Mürtschen Alp, in the canton of Glarus, from 1854 to 1861, but the mines have since been abandoned as unremunerative. The author remarks that during the Permian epoch conditions very favourable to the deposition of copper must have prevailed, whether by vapours laden with copper ascending from the interior of the earth, and depositing that metal in the rocks, or by salts of copper being

dissolved in water and precipitated and accumulated; but why it was precisely at the Permian period that the main deposits of copper originated is a question unsolved by the theory of aqueous deposition.

In the Brown Jura iron ore was formerly worked on the Oberlegli Alp, in the Klönthal, and in the valleys of Gentel and Lauterbrunnen, and at Gonzen, where it occurs along with black manganese ore, carbonate of manganese, heavy spar, fluor-spar, and iron pyrites. Petroleum is found in the lias marls to a great extent, but more in the Swabian Alp than within the political boundaries of Switzerland.

The carboniferous strata of Switzerland, limited as they are, bear witness to the very wide distribution of the plants of that epoch. The flora of the whole of Europe at that time was mirrored in the little island which formed the Switzerland of the carboniferous period. Even of the 300 species of coal-plants which have been found in America, about half occur also in Europe. This remarkable fact is not difficult of explanation if we consider that the flora of that epoch consisted chiefly of flowerless plants whose exceedingly minute spores would be easily swept away by the winds. Further, there was evidently a great uniformity of climate all over the globe, the chief amount of heat being due to the earth itself rather than that of the sun. The vegetable species in question would also encounter no opposition from social plants, such as grasses, which have since overspread so large a portion of the earth's surface; nor would they serve as food to animals—an exemption which their nearest allies enjoy to a very considerable extent even down to the present day. The carboniferous formation of Switzerland furnishes also the most ancient Swiss fossil animal with which we are acquainted—a species of cockroach (*Blatta helvetica*) nearly twice the size of the house-pest which has become naturalised throughout modern Europe.

The saliferous formation of Switzerland contains deposits of rock salt from 30 to 60 feet in thickness, and evidently due to a dried-up sea, as proved by the numerous bivalve shells found—the muschelkalk or shelly limestone. During this period Switzerland was covered by a shallow sea inhabited by encrinites quite foreign to the present European fauna; molluscs, approaching much more closely to those of our modern seas, and some of them even belonging to living genera. In Aargau Mösch has enumerated 57 of these species. Nautili, which now exist only in the Indian Seas, made their appearance in the carboniferous rocks long before the true ammonites. Fishes and reptiles inhabited the sea of the Triassic or Saliferous period. Remains of the *Ichthyosaurus* and *Nothosaurus* have been discovered by Mösch at Schwaderloch. The Keuper, or uppermost member of the Trias, is largely developed in the canton of Basel, where our author has obtained 25 species of fossil land-plants.

Thus we have proof that at the time of the Keuper there was in this region dry land, which probably extended over the Black Forest and the Vosges. The character of the flora is greatly changed from that of the Carboniferous period. The *Lepidodendra*, *Asterphyllitæ*, and *Sigillariæ* have disappeared, and in their stead we find gigantic horse-tails and Cycads of the genera *Zamia* and *Dion*, whose nearest living representatives are to be found in South Africa. In similar strata in Wirtemberg, Bavaria, and even in North Carolina, we meet with the same genera, and even the same species, as in the Swiss Keuper. Animal remains are also not wanting. Though Prof. Heer sought vainly for insects in the Keuper marls of Rütihard, he has found in the black shales of Vadutz two species of beetles (*Buprestites pterophylli* and *Curculionites prodromus*). We must here remark that the Curculionidæ and Buprestidæ are of all insects the most compact in their textures, as is well known to practical entomologists. The elytra of some of the species often turn the point of a pin, and require to be perforated with the point of a penknife or with a fine steel drill. Hence the remains of these beetles are much better calculated to resist either mechanical injuries or chemical changes than other insects, and we must therefore not be misled by their relative abundance in certain geological formations.

Huge reptiles inhabited Switzerland at this period: a gigantic *Labyrinthodon*, a salamandroid Batrachian, a crocodile (*Sclerosaurus armatus*), a large *Mastodonsaurus*, a *Belodon* "very like the gavial of tropical America,"* and certain *Teratosauri* have been found at Schambelen, in the Liesthal, &c.

The exploration of the Lias formation at Schambelen has proved exceedingly fruitful; nineteen fossiliferous beds, some containing marine and others terrestrial fossils, have been examined by Prof. Heer. He infers that the rock must have been formed in a quiet bay, protected from the agitation of the waves by a chain of hills projecting into the sea, or by a reef of rocks. It vividly reminded him of a scene he had witnessed in the quiet bay of Gorgulto, at Madeira. At Schambelen 22 species of plants and 182 of animals have been discovered. The latter consist of 1 reptile, 11 fishes, 143 insects, 6 Crustacea, 17 Mollusca, and 4 Radiata. The reptile, indicated merely by a tooth, was probably an *Ichthyosaurus*. Schambelen is the only locality on the continent of Europe where so many primæval insects have been preserved. In England the Lower Lias has indeed yielded 56 species, but they are in a worse state of preservation and less varied than those of Schambelen. The species found are distributed among the different "orders"—conventionally so-called—as follows:—Orthoptera, 7 species;

* This is a singular use of the name "gavial," which is generally restricted to *Crocodilia* of India and Australia.

Neuroptera, 7; Coleoptera, 116; Hymenoptera, Hemiptera, 12. Lepidoptera and Diptera are totally wanting, and the only Hymenopterous relic is a single wing. The author justly remarks that the hard elytra of the beetles may account for their relative numerical superiority, "but as the delicate membranous wings of the *Termites* have been preserved, there can be little doubt that if Lepidoptera had lived in the lias island, and fallen into the water, some traces of them would have been preserved." The absence of Diptera was a truly enviable feature of those days. Among the beetles the most striking feature is the relative abundance of the Buprestidæ, which we have to some extent explained; the paucity of weevils, due doubtless to the comparative scarcity of the seeds and fruits on which they feed; the total absence of long-horns, evidently not from a corresponding reason, as, like the Buprestidæ, they feed on wood; of ladybirds and of Brachelytra (devil's coach-horses). The scarcity of the Lamellicornes (dung-beetles, &c.) points to the want of ruminant animals, upon whose dung they prey. From a consideration of the plants and animals we may infer that the climate was at least semi-tropical.

In the Jurassic period "an immense sea extended over a great part of Europe, and in Switzerland only a few islands and coral-reefs rose above its surface." The islands of this period exhibit a flora having many links of affinity with that of the Lias, though the species are very distinct and deciduous leafy trees do not yet appear. The woody vegetation consisted chiefly of Conifers and Cycads, of species similar to some now flourishing in the southern hemisphere. The total number of species was small, as in the coral groups of the Pacific. The fauna of the Swiss Jurassic islands, as far as yet known, presents merely a few reptiles. If we complete the picture by a reference to contemporary deposits at Solenhofen, and in England and France, we find this period characterised by the appearance of the most ancient bird of the pristine world (*Archæopteryx macrura*), once regarded as a connecting-link between reptiles and birds. A dozen species of the Pterodactyle have been found at Solenhofen, and preyed doubtless on the gigantic dragonflies and large grasshoppers. The existence of dragonflies and of water-scorpions proves the existence of fresh-water lakes and pools. Longicorn beetles and Buprestids show that the islands were well wooded. The first Lepidopterous species, *Bombyx antiqua*, makes its appearance. The Jurassic strata of England have yielded remains of eighteen small mammals, belonging to the marsupial class which has in modern times its almost exclusive locality in Australia. But earlier traces of mammals have been found in the Trias of Richmond (Virginia), and in the Upper Keuper of Wirtemberg and England. The distribution of land and water in Central Europe at this period was remarkable. A large irregularly-shaped island comprised Bohemia, a narrow strip of

Central Germany, Baden, part of Eastern France, and Belgium. Central France, Holland, North Germany, the east of England, the middle of Switzerland, and the valley of the Danube and part of Northern Italy were the bed of a sea, whilst to the west a large island extended from Liverpool to the mouth of the Loire. After the Jurassic epoch, Prof. Heer is of opinion that *a transformation of the whole of organic nature had taken place.*

In the cretaceous period the central European island was greatly enlarged, and connected with Western England and Southern France, though the places where London, Paris, Vienna, Berlin, and Amsterdam now stand were still under water. The Swiss cretaceous marine fauna has two-thirds of its species in common with the Southern French (Mediterranean) sea, and only one-third in common with the Franco-Britannic sea. No remains of the land flora of this period have been hitherto discovered in Switzerland. From fossils found in other parts of Europe it appears that true leafy trees, such as poplars, figs, laurels, and myrtles, had now made their appearance.

In the Eocene period Europe must have possessed a nearly tropical climate and every condition favourable for the development of a rich fauna and flora. Evergreen forests of figs and Sapindaceæ, myrtles, and palms were inhabited by *Palæotheria*, musk deer and monkeys. Ferns have become scarcer than in the Cretaceous age, and Cycads have disappeared. The general character of the flora is Indo-Australian, though the peculiarly Australian types are less predominant. Mammalia are met with for the first time in Switzerland. There have been discovered 24 species of Pachydermata, 12 Ruminants, 1 Rodent, 8 Carnivora, and 1 Primate. All the species are distinct from those now living, and of the 25 genera represented only 4 have been handed down to modern times. One of the serpents was a python similar to those in India, and about 10 feet in length.

We arrive next at the Miocene epoch, and find the configuration of the European continent more nearly approaching its present state. Britain is united with France; Holland, Belgium, and Westphalia are still covered by the North Sea. The Mediterranean covers Egypt, spreads over Mesopotamia, and is probably directly connected with the Caspian on the one hand, and with the Indian Ocean on the other. Several isthmuses connected the European and the African coasts, and thus the African elephant, the hippopotamus, and the spotted hyæna are found in Sicily. The British Islands formed part of a great continent stretching across the Atlantic, and probably united to America. We thus perceive that Prof. Heer does not admit that vast antiquity of the existing oceans which many geologists and zoogeographers uphold.

As far as research has gone Switzerland "takes the lead of all countries in its magnificent specimens of the Miocene flora, of which no fewer than 920 species have been collected within

the territories of the Republic." The country appears to have been a tropical forest region somewhat resembling the valleys of the Orinoco and the Amazon. Of many plants whose remains have not actually been found, there is indirect evidence in the presence of certain insects. The following passage is of a nature so instructive to the student that we quote it in full:—"Although the species of insects of the Miocene are distinct from those now living, they are frequently so nearly allied to them as to permit comparisons and inferences. Thus a *Galeruca* found at Œningen enables us to assume that pond-weeds existed there, and in the same way the presence of a forget-me-not is indicated by a small and elegant *Monanthia*, that of brambles by a *Syromastes*, that of nettles by a *Heterogaster*, that of a viper's bugloss by a *Pachymerus*, that of a trefoil by a *Clythra*, that of thistles by a shield-beetle as well as by a *Glaphyrus*, and that of a figwort by a *Cionus*. Other insects give us intimation of flowery meadows: the *Syrphi*, *Anthomyæ*, and *Malachii* no doubt sunned themselves upon the flowers, and the bees and humble-bees of Œningen collected their nectar just like their relatives of the present day. The dung-beetles also announce the presence of grassy meadows."

Œningen, Prof. Heer calculates, must have possessed a flora at least twice as rich as that of any Swiss region of similar extent at the present time. Eleven species of palms flourished in Switzerland, whilst only a single species—the dwarf fan palm—maintains a footing in the extreme south of Europe. Bignonias twined about the trees of the dense forests; species of cinnamon, laurels, myrtles, and magnolias were conspicuous. "If we bring together, according to their native countries, the living homologues of the Swiss Miocene species, we find that 33 species live in America, 16 in Europe, 12 in Asia, 3 on the Atlantic Islands, and 2 in Australia. If we extend the comparison to both homologous and analogous species, we obtain the following numbers:—Of the species most nearly resembling the Swiss Miocene species, 83 live in the Northern United States and 103 in the Southern United States, 40 in tropical America, 6 in Chili, 58 in Central Europe, 79 in the Mediterranean zone, 23 in the temperate, 45 in the warm and 40 in the torrid zone of Asia, 25 in the Atlantic Islands, 26 in Africa, and 21 in Australia."

Hence the floral affinities of Miocene Switzerland must be pronounced to be, in a predominating degree, American.

Of insects we have 876 Miocene species belonging to Switzerland or its immediate environs. Of these, 543 species are Coleptera, 81 Hymenoptera, 3 Lepidoptera, 64 Diptera. Of the latter the majority are gnats and midges. Among the beetles the Curculionidæ and the Buprestidæ* predominate. But though

* We note that the term "gold-beetles" is used as an English name for the Buprestids. It and the corresponding German word "gold-käfer" have hitherto been applied to the Cetoniadæ.

we are hence warranted in concluding that they were more numerous in the Swiss Miocene fauna than in that of the present day, it is, from reasons already mentioned, unsafe to pronounce on the proportion which they may have borne to other and more fragile families. No true Cetoniæ are known, but the allied genera *Trichius* and *Valgus* have occurred. All the Melolonthidæ are rare, but one species, *Lepitrix Germanica*, represents a genus now confined to the Cape of Good Hope. Two of the water-beetles found surpass in size any species now living. It is remarkable that one of the water-bugs found at Œningen is on a similarly gigantic scale. The Lepidopterous order is scarce at Œningen, as in all other localities for fossil insects. It is probably the youngest order, and was very sparingly developed in bye-gone epochs.

In the Miocene of Switzerland and Œningen we find 6 salamanders, 8 frogs and toads, 6 serpents, 21 tortoises, and 3 crocodiles. One of the salamanders of this epoch is the species described by Scheuchzer as "*Homo Diluvii Testis*."

Six species of birds have been recognised, and Prof. Heer possesses a fine fossil feather from Œningen.

Mammals abound, especially Mastodons, *Dinotheria*, five species of rhinoceros, the *Anchitherium* and *Hipparion*, genera forming a transition to the horse of the present time, eleven species of swine, and *Hynælorus Sulzeri*—a connecting-link between the cats and the hyænas, but considerably larger than the Bengal tiger. *Hyænodon* also combines the characters of the hyænas and the cats with some approximation to the marsupials. *Galecynus palustris* appears to connect the dogs and the civets. Tailless long-armed apes have also been found belonging to the most highly developed group of the "Quadrumana," an order which we regret to see Prof. Heer still retains.

From a careful consideration of the flora of Miocene Switzerland we may conclude that its temperature must have been very similar to that now enjoyed by Louisiana, North Africa, and the Canaries, with a mean annual temperature of 68° or 69° F. Even at Spitzbergen the Miocene flora resembles that now existing in Northern Germany, and indicates a mean temperature of 46° F.

But we must now take leave of the Miocene epoch, lingering traditions of which, if man existed so early, may have given rise to the "golden age" of mythology. Even before its close, however, a deterioration of climate became manifest, and in the next, or Quaternary period, the temperature of Switzerland and of Europe in general sunk to its present level. In the interval between the close of the Miocene and the formation of the Lignite, Prof. Heer thinks that the great western continent, which may have been the Atlantis of Plato, was submerged. Thus the connection between Europe and America was cut off, and perhaps the upheaval of the Alps took place at the same

time. This brings us to the contemplation of that awful phenomenon known as the Glacial epoch, or rather epochs; for two distinct attacks of glaciation, each lasting probably for thousands of years, are traced by Prof. Morlot from the mode of dispersion of the boulders of the Rhone Valley. In the interval between the two Glacial periods occurred, in Switzerland, the formation of the Lignites. Even this period has left traces of an important fauna. The remains of two huge oxen, the urus and the auerochs, both mentioned by Seneca and Pliny, can be traced to this period. Bones of the Irish elk have been found at Isteinerklotz. The woolly rhinoceros and the mammoth have been found in not a few Swiss localities, the latter of which made its appearance at the end of the second Glacial epoch. As to the existence of man in the Interglacial epoch, Prof. Heer regards the evidence as doubtful, as far at least as Switzerland is concerned.

Thus far we have given a brief and necessarily imperfect outline of Prof. Heer's view of the Palæontology of Switzerland from the Carboniferous to the Glacial epoch. It has been, of course, impossible for us to point out more than a mere fraction of the interesting facts here brought together, for no small portion of which the world is indebted to the untiring zeal and acute observation of the author himself,

Now, however, we enter upon the lessons to be learned from the phenomena placed on record—a matter where inference, not to say conjecture, is brought into play. It may here be needful to call attention to a peculiarity of the edition before us. It does not appear to be a literal translation from the original German, but bears rather the air—in certain passages at least—of an abstract or paraphrase. Indeed we are told, in the Preface, that both the German and a French edition were placed in the hands of Mr. Dallas for translation, and that the editor, Mr. Heywood, in revising the MS. was principally guided by the French version. This is to be regretted, since the responsibility of the opinions contained in the latter part of the work becomes thus divided. In our opinion the German edition should have been literally translated, and any valuable peculiarity of the French should have been added in the form of annotations or appendices.

Among the speculative questions to which the latter portion of this valuable work is devoted, we may select especially three —the causes of the glacial epoch, the first appearance of man, and the origin of species.

In the earlier periods, from the Carboniferous to the Tertiary, the earth's climate must have approached most nearly to that of the present torrid zone, and Prof. Heer finds no satisfactory evidence either of increase or decrease of temperature during this immense period; nor, until the Tertiary epoch, is a distribution of heat in zones perceptible. Even then the decrease of tem-

perature towards the poles was much less marked than at present.

As regards the causes of those alternations of temperature from the Lower Miocene to the Glacial, our author refers—but with only partial approval—to the influence of different distributions of land and water upon the earth's surface. The Arctic Ocean was probably, during the Glacial epoch, in connection with the Baltic, and the Sahara was—according to Prof. Escher de la Linth—submerged by the sea.*

Prof. Heer then examines four of the hypotheses advanced for the explanation of the phenomena in question. These are—Change of climate from the diminution of the earth's natural heat; modification of the sun itself; change of position of the earth with regard to the sun; and irregularity of temperature in ethereal space.

The first of these hypotheses would agree well with a constantly decreasing temperature, but it is utterly inadequate to explain a double period of intense cold with a milder epoch intervening, and succeeded by a more genial climate. Dr. Blandet's hypothesis, of a progressive decrease in the solar heat, is equally inadmissible. The third hypothesis, Mr. Croll's,† which seeks the cause of such alternating extremes of temperature in the periodically varying eccentricity of the earth's orbit, is rejected by our author on the ground that the plants and animals preserved in the rocks by no means confirm Mr. Croll's theory. The fact that the northern half of the globe has warmer summers than the southern atmosphere is ascribed to the different distribution of land and sea, the writer forgetting that the summer of the southern hemisphere is shorter than that of the northern, from the greater orbital velocity of the earth when in perihelion. Prof. Heer seems to lean to the fourth and last hypothesis, which seeks the cause of warm and cold periods in the temperature of different regions of space. "The Miocene period may be compared to the summer, the Glacial period to the winter, and the existing geological age to the spring of the planetary system."

But it is inconceivable that the temperature of different regions of space can vary, save as far as is due to the greater or less number and propinquity of stars radiating heat. Now if our earth, or rather our planetary system, passed so near such stars as to raise the average temperature of Europe by 16° F., their attraction would undoubtedly have exerted a very decided influence on the orbits of the earth, the other planets, and their satellites. Yet of such perturbations astronomers can find no traces. It will at once appear that, on the first, second, and fourth of these hypotheses, glaciation would occur in both the

* There is grave reason to fear that the schemes for letting in the ocean upon the Sahara might have disastrous effects upon the climate of Europe.

† Quarterly Journal of Science, vol. iv., p. 421, and vol. v., p. 309.

northern and southern hemispheres simultaneously, as Mr. Belt supposes. On Mr. Croll's view the two hemispheres would be subject to alternate glaciation.

The first appearance of man in Switzerland is traced, in an Appendix from the pen of Prof. L. Rütimeyer, of Bâle, to the Interglacial period. Pointed rods, carbonised along with the other constituents of the lignite, yet bearing evident traces of human labour, have been discovered among the coal from the Schöneich pit, at Wetzika, near Bâle. But if the first traces of man in Switzerland are thus shown to be Interglacial, his first appearance in the world is much more likely to have fallen in the Miocene epoch. Unless our race had been already widely distributed, and had made some advance from its rudest condition, it would probably have perished in the first Glacial epoch, under circumstances so unfavourable to any being of the order Primates.

Let us next turn to the question of the origin of vegetable and animal species.

Prof. Heer appears as an evolutionist, but as a non-Darwinian. He admits that during the Geological epochs, which he has so ably and graphically described, species have arisen, flourished, and passed away, giving place to others. These changes he does not ascribe to a series of miracles. On the contrary, he holds that new species are more likely to have been of organic than of inorganic origin, and even suggests that certain extinct species may have been the ancestors of modern forms inhabiting the same or adjacent regions. But he does not receive "natural selection" as the cause of the mutations of species. He denies that all living beings are necessarily and constantly undergoing a process of development, or a slow and uniformly progressive transformation. He considers that animals display a stability not only in their physical constitution, but also in their instincts, which he regards as decisive with reference to the continuance of specific characteristics.

It has been objected to this view that our observation of the habits and "instincts" of animals, especially of insects, is far too recent and too imperfect to enable us to decide whether these instincts are fixed or stationary. Prof. Heer seeks to elude the force of this argument by saying that certain species—*e.g.*, of ants in Switzerland—have precisely the same "instincts" as the same species in England or in Sweden. But as England has been separated from the Continent for probably 100,000 years it may be inferred that the instincts of these Swiss and English ants, both belonging to one species and derived from one common stock, must have remained stationary for at least such a length of time. To this certainly ingenious argument we may reply that the habits, and consequently the instincts of insects, at least in numbers of cases, are not specific, but common to entire genera, and even extend over larger groups. We need

therefore feel little surprise if, under circumstances mainly similar, we can perceive no modification, even during such long periods of time. Further, the sea does not absolutely prevent the passage of insects from the Continent to England. Hornets, wasps, winged ants, &c., may cross a strait of 20 miles in breadth as readily as butterflies and locusts, both of which have been known to fly much greater distances. We may further point to distinct instances where "instincts" have been found capable of variation, and where changes have been made in accordance with modifying circumstances. In a book which is in the hands of every naturalist* we find instances of alterations and improvements in the nests of birds.

Prof. Heer maintains that "if instinct were the result of education it would at the same time be capable of attaining perfection, and, in the case of insects gifted with the most wonderful instincts, changes might be expected more rapid in consequence of the very limited period of individual existence of each insect." Now, this "very limited period of individual existence" is, we maintain, precisely the reason why changes are so slow. The longer any animal lives the greater opportunity it has to profit by experience, and the more probability there is that its fellows can observe and profit by any improvement upon which it may have come. If we consider how slow has been the progress of man—how often even the most highly-organised varieties of our species have been at a stand-still, or even retrogressive—need we wonder that in animals far lower the movement may escape our observation?

Prof. Heer considers that we are still in the dark as to the fundamental conditions of the transformation of types which he admits to have taken place. Believing the changes to be sudden instead of gradual, he approximates to the position of Prof. Mivart.

The following passage from the Editor's Preface should not be overlooked :—

"In Ancient Tertiary strata the chestnut (*Castanea atava*) had leaves distinctly toothed, but devoid of any points, and from the primary vein of the leaf curved secondary veins sprang at a distance from each other.

"In the Middle Tertiary strata the secondary veins of the chestnut leaf approximated to one another, and teeth protruded and were more numerous.

"In the latest Tertiary strata the secondary veins of the chestnut leaf were still nearer to each other, and almost rectilinear, while the teeth of the leaf had become set with thorns, as in the sweet chestnut (*Castanea vesca*) of our own times."

Is not this a striking case of that gradual transformation which Prof. Heer is unwilling to recognise?

* Wallace, on Natural Selection, pp. 224 to 230.

The edition before us is abundantly illustrated. There is a geological map of Switzerland; there are a series of small charts showing the arrangement of land and water in Central Europe during the successive epochs; there are a number of representatives of the fossil vegetable and animal forms described in the text; and, lastly, a set of imaginary landscapes, showing the characteristic fauna and flora of Switzerland at each period.

Tonquin. Report by Sir B. Robertson (Her Majesty's Consul at Canton) respecting his Visit to Haiphong and Hanoi, in Tonquin. 1876.

So little information has been made public in recent years respecting the almost unknown kingdom of Tonquin, in the Indo-Chinese peninsula, which, through the intervention of the French, has just been partially opened to foreign intercourse, that we venture to think a brief *resumé* of some portions of this very interesting Report to the Foreign Office may not be unacceptable to our readers.

We must premise that last spring Her Majesty's Minister at Peking directed Sir Brooke Robertson, H.M.'s Consul at Canton, an officer of long standing and great experience, to visit Haiphong and Hanoi, the latter the capital of Tonquin, to ascertain their commercial capabilities. Haiphong is situated on the right bank of the Cua-cam, a branch of the Song-koi, or Red River, on which is Hanoi at a distance of 145 miles from the sea. Haiphong is connected with Hanoi either by the River Cua-cam or by canals: after proceeding some 25 miles by this route boats turn into another canal, and after going some 77 miles enter the main river, the Song-koi, and so on to Hanoi, a distance of about 45 miles. At Haiphong there was little to be seen and not much to be learned, for what business there is is done at the capital, where the native and Chinese merchants all reside, and thither Sir B. Robertson proceeded as soon as he could make arrangements for the journey in a small steam-launch. For some time but little was to be seen of the country, the banks of the canal being too high; but from what could be seen it appeared to be fertile, and planted with maize and sugar-cane, sweet potatoes, and other products of those latitudes. On debouching, however, into the Song-koi, a better view was obtained, and there the bamboo and other trees, with villages and fields, made together a scene of much natural beauty, and gave evidence of a high state of cultivation. There is a fair amount of boat and junk traffic; from the stake-nets, &c., fish evidently abounds; rafts of wood and bamboo were frequently met with; and altogether there was evidence of a contented and well-to-do

population. From December to May the Song-koi, or Red River, is very low, but about May the melted snow and ice from the mountains of Upper Tonquin and the Chinese province of Yünnan come down, and the river rises rapidly, attaining a height of 30 feet, and often carrying away the banks and flooding the country.

The approach to the city of Hanoi, the capital of Tonquin, is very fine, for a turn of the river—at that point over a mile in breadth—brings into view the town and the settlement, on the construction of which the French are expending much care. The city rises gradually from the river, and, being embedded in trees and foliage, it has a charming appearance on a bright and sunny day. That part of Hanoi where the citadel—so gallantly captured by Lieut. Francis Garnier on November 30th, 1873—is built is somewhat higher than the other quarters of the city, the ground rising gently from the banks of the Song-koi to the height of about 180 feet above the level of the sea. A brick wall, about 3 feet thick and 12 feet high, comprising twelve bastions connected by curtains, surrounds the citadel, which forms a perfect quadrilateral of some 3600 feet on each side. Inside this citadel are the houses of the Governor and other high officials. Besides these and some barracks, there are but few houses; in the centre there is a very peculiar tower on a raised base, some 20 feet high, and on the top of this tower the national flag is hoisted.

The town of Hanoi—the population of which, we may note in passing, is estimated at from 150,000 to 200,000 Tonquinese and about 2000 Chinese—is situated between the citadel and the river, and extends beyond the former in a westerly direction. The streets are wide, and the houses good and well-built of brick in some of the principal thoroughfares, but there are numerous streets where the shops are merely mat erections, in the usual Tonquinese style. With one exception—the main street from the river, where the Chinese live—the streets are unpaved, and in wet weather almost impassable from the depth of the mud; but as shoes are an exception with the natives, this drawback to European comfort does not affect them.

The sedan-chair of China—so convenient for locomotion—is unknown, the high authorities and those who can afford it being carried in a hammock of silk or hemp net-work, suspended on a pole from the shoulders of two or four bearers, and closed with silk or cotton curtains. There is a great deal of stir and movement in the streets; the shops are well-stocked with articles of native workmanship—especially bamboo-work in all its varieties; silk and cotton piece goods; paper, pewter, and glass ware; boxes; drums of all sizes, richly painted, lacquered, and gilt; and coarse crockery and china-ware. Fowls, ducks, geese, oxen, and pigs are both plentiful and cheap, with abundance of fruit, bananas, vegetables, and fish. Rice is the staple food of the

people, who look well-fed and healthy, and have a fair amount of muscular development. The women, however, appear to be the dominant class, for they work like the men as coolies and labourers, and preside in the shops. They are generally good-looking, carry themselves gracefully, and, but for the practice of blackening their teeth, many might be called handsome. In manner the Tonquinese are quiet, and, owing perhaps to the arbitrary government they are under, subdued and indolent. Mr. N. B. Dennys, who visited Tonquin last spring as the delegate of Hong-kong General Chamber of Commerce, gives us, in his Report, some more details respecting the women of Tonquin, and it will be seen that he is at variance with Sir Brooke Robertson on the subject of their good looks. "The dress of the women," he writes, "is picturesque, their figures are good, but their faces, as a rule, are by no means pretty, and very dirty-looking. They wear trowsers, with a square piece of silk over the breast, fastened behind. The better sort wear a long dress over this. Their heads are covered with cloth turbans, with the hair so arranged as to seem as if it was in a small net at the back. The teeth of both men and women are, as amongst other betel-chewing races, perfectly black, and their lips a bright red. The ordinary dress of the men is a long coat and trowsers, with a conical hat, from either side of which hang long pieces of silk."

Returning to Sir B. Robertson's Report, we note some of the leading geographical features of the country. The principal river in Tonquin, the Ho-ti-kiang,—better known now as the Hung-kiang in Chinese and the Sung-koi in the native language, both names meaning Red River,—has its rise in Thibet, and is navigable from Mang-hao, the last city in the Chinese province of Yünnan, down to the sea, a distance of 414 miles. Mang-hao is the *entrepot* where goods are shipped to and from Tonquin, the mart being a more northern city, called Mang-tsze, which is also situated on the banks of the same river. The Sonk-koi divides into various branches, the two southern ones meeting above Hanoi; on one of them stands the town of Niuh-brinh, and on the other Nam-diuh and Hung-yen. These two branches communicate by canals, on one of which is situated the town of Fouli. It is not necessary further to describe the various branches, &c., of the Song-koi; but as it may become of very great importance, if the French ever succeed in opening up a trade with South-Western China, which Sir B. Robertson evidently considers by no means an impossibility, the following remarks of his are not without interest:—"I saw the peculiarly constructed boats, of great length and drawing little water, employed in ascending the river as far as Mang-hao and Mang-tsze, both large trading cities in Yünnan, and I closely questioned one or two people who had been that route, and they stated that no difficulties existed. That the River Song-koi is available,

therefore, for the transit of the reported rich ores and produce of Yünnan and Tonquin, seems certain, and on the removal of three obstacles alone a large and profitable trade may be said to depend, viz., the consent of the Governments of Tonquin and China to open the mines, and the clearance of the frontier of refugee Chinese and leviers of black-mail. All these accomplished, the rest is easy. If the Song-koi, or Red River, opens a route to Yünnan and Kwangsi, that *via* the Shan States and Bhamo is not the less available, and the two offer equal advantages, nor is there any reason why their interests should clash; on the contrary, for these points of exit are distant, and one gives geographical facilities which the other does not."

Sir B. Robertson found it difficult to obtain information regarding the commercial products of Tonquin; but he met with a native, called Patrus Trueong Vinkky, who was an *employé* of the French Government at Saigon, and had been sent to enquire into the trading resources of the country. He represented Tonquin to be rich in products of all kinds, having coal, copper, and tin mines, as well as gold and silver. Its agricultural capabilities are immense, and silk is abundant and cheap,—in fact, not much more than one-third of the price it is in China: it appears, however, at present to be much coarser in thread than the Chinese, and very badly reeled, and the fabrics made from it are of very poor quality. From all he could learn, Sir B. Robertson concluded that Tonquin was a very rich country, and, with the establishment of good relations with the Government, might be made a very profitable one to western nations.

The Theory of Sound in its Relation to Music. By Professor Pietro Blaserna, of the Royal University of Rome. London: Henry S. King and Co.

This volume of the International Scientific Series will be acceptable to all musicians who wish to know something of the manner in which sounds are produced, and the laws regulating them. The author has combined his information in the most happy manner, and produced a work useful both to the students of physical and musical science.

The earlier chapters treat on vibrations, and the various well-known experiments for their demonstration. After discussing the production of sound by means of the vibration of solid bodies, the subject of sounding-pipes is explained, and their application in the construction of wind instruments. The description of the manner in which a single elongated tube is able to produce several notes, by overblowing, aided by opening communications with the external air by means of keys, as in the flute and clarionet, is correct; but the singular error has arisen

of attributing the same action to the pistons of brass instruments which do not open holes in the tube, but by means of valves add additional lengths of pipe, and so produce different fundamental notes, the harmonics of which are formed by the lips and pressure of breath of the performer.

Fuller information respecting the structure of musical instruments would have been very acceptable to a large class of readers, especially as information on this subject is not easily attainable.

Several chapters are devoted to subjects almost entirely musical, such as ratios, consonant and dissonant chords. The nature of scales, ancient and modern, is explained in considerable detail. The "Equal Temperament" does not find much favour with Prof. Blaserna: he accepts it as a necessary evil, but considers that a key-board of twenty-four notes to the octave would just be able to produce a satisfactory effect.

The chapter on *Timbre*, or quality of sounds, is particularly interesting, and contains descriptions of the *Phonautograph* of Scott, and the equally valuable *Harmonic Detector* of König.

Histological Demonstrations, being the Substance of Lectures delivered by George Harley, M.D., F.R.S. Edited by George T. Brown, M.R.C.V.S. Second Edition. London: Longmans and Co.

Works containing the practice of accomplished manipulators in any branch of Science are always welcome, and the present little book is no exception. The carefully edited notes of Mr. Brown have in this edition had the advantage of Professor Harley's revision, and the result is a collection of practical information of the utmost value to medical and veterinary students.

The portion of the work devoted to the description of the instrument, its use, and the general processes employed in the preparation of tissues for microscopical examination, is somewhat small, only twenty-seven pages being devoted to these subjects; however, this department has been so fully treated in the copious works of Beale, Carpenter, and others, that greater extension was hardly needed, and more space has been left for the special subject of the work.

Of the treatment of this part it is hardly possible to speak too highly.

The tissues, from the most elementary to those of the highest elaboration, are described briefly, yet with sufficiency of detail to aid the student; special directions are given as to the best mode of manipulating, and a diagram or figure given with each, to call attention to what should be observed; no less than two

hundred and twenty woodcuts are inserted in a book of two hundred and seventy-four pages.

The examination of diseased tissues is fully demonstrated. This portion of the work will be useful not only to the student, but may be consulted with advantage by the medical practitioner, who will find, in a compact form, information which would otherwise require a search in many and not easily procured publications.

Animal and vegetable parasites have also a fair space devoted to them.

The woodcuts are well executed; the only defect of the work is the absence of references to authorities where further information is to be obtained.

A Letter addressed without permission to the Astronomer Royal, explaining a New Theory of the Solar System, and placing Newton's Theories upon a Physical Basis. By G. T. Carruthers, M.A. London: Longmans and Co.

We cannot give the reader a better idea of the views advanced in this little work than by quoting from an abstract which the author has appended:—

"The earth and planets are supported by the atmosphere or its watery vapour, in the same way as the weight on a safety-valve is supported by the steam in a boiler. The rotation of the earth is caused by the impacts of the atmosphere upon the earth, *i.e.*, by a constant series of blows from the sun of 2116 lbs. upon every square foot; the number of feet passing in a second being, as we know, enormous.

"The orbital motion of the earth is caused by the rush of cold air at night towards the sun, as the air in a room rushes towards the fireplace. Magnus has shown, by experiment, that such a rush of air upon a revolving cylinder causes it to move in a lateral direction, similar to that in which the earth moves.

"The issue of watery vapour of very rare density takes place from the North Pole, where there is little rotation. The atoms of vapour gradually accumulate and form condensations of vapour at great distances from the North Pole, when the force with which they were expelled is expended. Thus the earth has formed the moon, and the sun the planets.

"The atoms when condensed are urged downwards to the Equator, and receive there the rotary impacts of the sun's vapour, which compel them to rotate in the same direction as the sun. The planets may thus be supposed to be at the centre of gravity of great cones extending northward, which rest upon the great cone of the sun's vapour. Hence the motion of the

earth may be likened to that of a ball suspended from a string and twisted around, and the earth goes upon its orbit as one cone rolls round the surface of another. This theory explains, satisfactorily, therefore, the observed motion of the earth, which seems to be going forward by twisting itself backwards in a way quite different to our common notion of the manner in which a sphere or carriage-wheel should roll onwards.

"The cones are masses of vapour at its greatest density for the temperature, of sufficient strength and power to sustain the planets in their places, and the distances of the planets accord with the volumes of steam which sustain similar weights.'

The Puzzle of Life and how it has been put together, a Short History of Vegetable and Animal Life upon the Earth from the Earliest Times. By Arthur Nicols, F.R.G.S. London: Longmans and Co.

We have here an elementary treatise on geology and palæontology adapted to the comprehension of young children. The language is plain, the descriptions are lucid, the illustrations apt, and the broad facts of the science are very correctly stated. The work, too, is perfectly free from all attempts at fine writing, and from those peculiar passages we occasionally meet with which prove that their author is thinking more of himself than of his subject. Trains of argument are of course absent, and the thread of the subject is not overlaid with a perplexing multitude of details. Nor, on the other hand, has Mr. Nicols deemed it judicious to obscure his theme with "the mist of romance." He remarks, and we believe quite truly, that "children soon begin to look with contempt on fiction when applied to natural philosophy, and I would not consciously impose on them the grave task of unlearning anything." But whilst we feel bound to express our approbation of the manner in which the author has carried out his undertaking, we cannot help expressing a doubt as to the reception the book will meet with among children of an average class. From the Dedication and from some expressions in the Preface we should infer that the author has brought the subject-matter of these pages before a number of children in *viva voce* lessons or conversations, and has succeeded in awakening their interest and in reaching their understanding. But children are confessedly reached much more readily in this manner than through the medium of printed paper. We wish the book success as at any rate an attempt to lay before the young fact instead of fiction,

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Problems and Examples in Physics. An Appendix to the Seventh and other Editions of Ganot's "Elementary Treatise on Physics." London: Longmans and Co.

THE author of this pamphlet, Mr. E. Atkinson, remarks in his Preface that—"The present book, consisting of a series of numerical problems and examples in Physics, is based upon a similar one contained in the French edition of the work. But I have been able to use only a small proportion of the problems contained in that Appendix, as the interest of the solution was in most cases geometrical or algebraical. Hence I have substituted or added others, which have been so selected as to involve in the solution a knowledge of some definite physical principle."

We consider the substitution thus referred to by the author a decided improvement.

The Applications of Physical Forces. By AMEDEE GUILLEMIN. Edited by J. N. LOCKYER. London: Macmillan and Co. 1877.

THIS forms the companion volume to the "Forces of Nature" of M. Guillemin, a translation of which appeared in this country five years ago. The present work is published in the same form, with numerous and elaborate woodcuts and coloured plates: the one deals with pure science; the other with some of the more prominent applications of the sciences.

The work is divided into five books, which treat respectively of the applications of the phenomena and laws—1. Of Weight. 2. Of Acoustics. 3. Of Light. 4. Of Heat. 5. Of Magnetism and Electricity.

We must take exception to the title of the first book: to speak of the phenomena of *weight* when we mean the phenomena due to a force, one of the effects of which is to produce the effect we call "weight," is surely an example of misplaced diction.

This book treats of plumb-lines and levels, pile-drivers, the pendulums of clocks, and balances. In the second section, of the hydraulic press, hydrometers, liquid levels, pumps, fire-engines, and air-pumps. The atmospheric railway is described, but no mention is made of the use of air-pumps for exhausting vacuum-pans. The applications of compressed air comprise the air-gun (which is surely the least important application) and the boring of tunnels by a compressed air-engine. Compressed-air railways are beginning to make their appearance in various cities: in Paris small parcels are conveyed from the Grand Hotel to the Place de la Bourse at the rate of 40 feet a second. The pressure of air is produced by the pressure of water (equal

to 15 metres of vertical height) in the reservoirs of the city. In London there are twenty-five pneumatic tubes representing a length of 18 miles. The tubes are 2¼ inches diameter, and are worked by alternately compressing and exhausting air by means of three steam-engines, each of fifty horse-power. In New York passengers are conveyed by the same means.

A curious misprint, twice repeated, occurs on p. 81; Hero's Fountain is printed *Nero's* Fountain: the same error occurs twice on the following page. A chapter on ballooning concludes the first book.

In the second book, on Acoustics, we have a useful section on "Acoustics applied to Architecture;" then a description of various musical instruments, ancient and modern, including a most capital account of the construction of the violin, "the most perfect of musical instruments. The back and ribs of the violin are made of a hard close-grained wood, usually beech; the upper plate or belly is made of a light wood, such as deal or cedar. The old violin-makers preferred Swiss pine for the belly and maple for the back. An effective woodcut (fig. 96) shows the various parts of the instrument. Although, as is well known, violins improve considerably by age, our author very justly remarks—"But it must not be forgotten that the beauty of the tone of an instrument of this kind depends, in great measure, on the talent of the artist in whose hands it may be. Nearly his whole skill, from this point of view, lies in regulating the pressure by which his right arm, or more properly speaking his right hand, directs the bow, and the clearness and force with which the fingers of the left hand press the string." The complex mechanism of the harp is well shown in fig. 109. An interesting account of the great organ at Primrose Hill, built by Bryceson Bros., is given in Chapter 5. This fine instrument embraces seven distinct organs, and the bellows and "vacuum pressure" are worked by an eleven horse-power steam-engine. There are seventeen compressed-air reservoirs, and two vacuum reservoirs. The echo organ—100 feet distant from the keyboard—is worked both by electricity and by vacuum pressure, that is, the pressure produced by air striving to enter a vacuum. The 32-feet pipes in the great organ in Notre Dame each require 70 litres of air a *second* to sound them, while the whole mass of compressed air in the organ amounts to 25,000 litres.

The third book treats of the applications of the laws of Light, and first of mirrors and reflectors, and of instruments in which mirrors are used, such as the sextant and goniometer, the heliostat and siderostat. A good account of lighthouse appliances follows: figures are given of the azimuthal condensing prisms used in the Sound of Skye, and of the peculiar form of lens used in the Tochindall lighthouse.

The chapter on the Microscope contains some beautiful coloured drawings, among which we may specially call attention to the

distribution of blood through the brain, and the section of the retina of a bird. A brief account of photo-lithography and chromo-heliography conclude this book.

The fourth book, on the applications of the laws of Heat, commences with a discourse on the art of warming, naturally followed by a comparison of different kinds of fuel. The steam-engine, in all its phases, receives very full treatment, and these seem to us to be the best chapters in the book—the most complete and comprehensive, and most carefully written and edited. A description of the condensed-air locomotive which is used in the St. Gothard tunnel, and of several kinds of road locomotives, concludes this part of the subject.

In regard to the application of steam to printing, it is mentioned that the first sheets printed by steam were struck off in 1814. Ten thousand copies per hour were struck off. Till lately the "Hoe ten-feeder" was the most rapid printing-press in existence: it can turn out 8000 copies of perfect newspapers per hour, but requires eighteen people to attend to it. The Walter press, however, by means of which "The Times" is printed, requires the attention of one man and two boys, and prints 12,500 copies per hour.

Some interesting statistics of steam-engines and railways will be found on p. 498. From these we learn, on the authority of Fairbairn, that the steam-power employed in England alone is equal to 3,650,000 "horse-power,"—equivalent to the labour of 76,000,000 men. In France there were in 1865 steam-engines having a united power of 242,209 horse-power, not including 4000 locomotives. At the end of 1876 the railways of the world had reached a total length of 176,141 miles, which is nearly seven times the circumference of the earth. The following numbers show the distribution of the mileage:—

Europe	83,864
America	82,335
Asia	6,822
Africa	1,675
Australasia	1,463

The next statement is somewhat surprising:—Out of 100,000 ships in Europe forming the mercantile marine, 4500 ships employ steam—a much smaller number than we should have imagined. The 100,000 must surely include a large number of coasting-vessels and other small craft. At the same time sailing-vessels are being very rapidly converted into steam-vessels; and while in 1861, out of 975 new ships, only 207 were fitted with steam, in 1874—out of 981 new ships—no less than 482 were steam-ships. In 1874 we had no less than 109 steam-vessels in the Navy, and of these sixteen were ironclads.

The fifth and final book treats of the applications of Magnetism and Electricity, and this constitutes more than one-quarter of

the entire work. Commencing with an account of the compass and dipping-needle, the subject of lightning-conductors is next discussed, and this is followed by the great subject of electric telegraphy. This receives very ample discussion, and all the newest improvements are described. The electric light is now employed far more generally than it used to be, and apparently it is far from expensive. The electric light which was employed in the clock tower of the Houses of Parliament was produced by a Gramme machine driven by a two horse-power engine at a rate of 320 revolutions per minute. It is stated that it produced "a light equal to 7000 sperm candles, at a cost of about one shilling per hour." The electric light, which is employed for many stage effects, at the Grand Opera in Paris is produced by 360 Bunsen elements. The application of this intense light to the lighting of towns has not proved a success: it is probable, however, that if a great elevation could be secured, it might be used with advantage. It would be interesting to try the effect of the light on the top of Strasburg Cathedral. A few general applications on electricity conclude the fifth book and the volume.

This new work of M. Guillemin is a useful adjunct to his former volume on the physical forces; it does not, however, present the same features of novelty to the general reader, because the former volume, on account of its popular nature, could not well avoid allusions to some of the practical applications of the forces which it described.

Acoustics, Light, and Heat. By William Lees, M.A. London and Glasgow: William Collins and Co. 1877.

Messrs. Collins are doing good service to the cause of scientific education by issuing a cheap and generally excellent series of text-books. There are two series of such books — an "Elementary" or Primer Series, and an "Advanced," usually by the same author. The elementary volume of the present work has been published for some length of time, and has been found a fairly useful book for science classes in schools. The work before us embraces all that can be required by the advanced classes in schools; it is well illustrated, and in the main clearly written.

An Introduction to the Theory of Electricity. By Linnæus Cumming, M.A., Assistant Master at Rugby School. London: Macmillan and Co., 1876.

This work embraces in a compact and well-arranged form many of the mathematical results which have been obtained by Prof. Clerk Maxwell and others. It commences with a chapter on "Physical Units," in which we find definitions of *Velocity*, *Acceleration*, *Density*, *Force*, *Momentum*, *Moment of a Force*, *Couple*. The latter is defined as follows: "Two forces which are equal in magnitude and parallel, acting in opposite directions, but not in the same straight line, are termed a *couple*." Work is defined as "resistance overcome through space." Kinetic energy as "half the product of the mass into the square of the velocity of a body." The second chapter is on the theory of the *potential*. The potential at a point is defined as "the work done in carrying a gramme from that point to infinity." The expression "difference of potential" is frequently used in electricity, and its precise meaning is this: that if the potential of any two attracting systems be measured, the difference represents the work done on a unit of mass moved from one point to the other. *Tubes of force* are "tubular surfaces bounded by lines of force." The ideas developed in this chapter are next applied to certain experimental results; such as the proof that electricity resides entirely on the surface of bodies. A number of problems both in Statical and Voltaic Electricity follow; Ohm's Law is discussed at great length, and finally we have an application of some of the preceding laws to magnetism. The work will be welcomed by all students of electricity. It supplies a distinct want, and supplies it well.

A Treatise on the Kinetic Theory of Gases. By Henry William Watson, M.A. Oxford: Clarendon Press. 1876.

This small work gives us, in a condensed form, the main results obtained during the last few years by Clausius, Clerk-Maxwell, and Boltzmann. It is from first to last mathematically treated, and will only be useful to the reader well acquainted with mathematical modes of thought. It is included within fifty-one pages, and embraces thirteen propositions with their proofs. We may take the tenth proposition as an example:—"A homogeneous gas being supposed to be constituted of moving molecules with any given number of degrees of freedom, required to find the ratio of the specific heat at constant pressure to that at constant volume."

Chemical and Physical Researches. By Thomas Graham, D.C.L., F.R.S. Collected and printed, for private circulation only, by James Young, F.R.S. and Dr. Angus Smith, F.R.S. Edinburgh: 1877.

It would have been difficult to offer more graceful evidence of friendship, and nothing would have touched and gratified Mr. Graham more, than the publication of this volume by his valued friends Mr. Young and Dr. Angus Smith.

Mr. Graham's work was peculiar, as his researches were mainly devoted to the elucidation of questions which occupy an intermediate position between chemistry and physics. Dr. Smith therefore wisely determined not to place the papers in strict chronological order, but to arrange them under three divisions, headed respectively "Gases," "Salts and Solutions," and "Unclassified Papers." The plan is a most fortunate one, for it well shows the gradual development of Mr. Graham's thoughts in each division of his work, and at the same time it exhibits its strength and coherence.

His earliest paper "On the Absorption of Gases in Liquids," published in 1826, is remarkable for ingenuity and close reasoning. In it he considers that "gases may owe their absorption in liquids to their capability of being liquefied, and that when gases appear to be absorbed by liquids they are simply reduced to that liquid inelastic form which otherwise, by cold or pressure, they might be made to assume; their detention in the absorbing liquid is owing to that mutual affinity between liquids which is so common. Faraday had shown, in 1823, that in the physical states of gas, liquid and solid, there was nothing of absolute permanency, and that any body may assume consecutively all these forms." Hence Mr. Graham concluded that those bodies which at the temperature of the atmosphere we experience to be gases, may be considered, without impropriety, as volatilised liquids, and he pointed out that it was not necessary that gaseous bodies —whose absorption in liquids he was explaining—should be presented to the liquids in a liquefied state, for the mere absorption of such gases by liquids occasioned their liquefaction. He gives as an instance the liquefaction which must accompany the absorption of steam, at 600° F., by sulphuric acid heated to the same temperature, while in order to liquefy the gaseous body in the ordinary way it would be necessary to cool it down through a range of nearly 400°. The absorption is, in fact, dependent upon "the affinity which occasions the miscibility of two liquids." In his last paper in the "Philosophical Transactions," published more than forty years afterwards, he refers to the liquefaction of *gases in colloids* in much the same terms, for he alludes "to the general assumption of liquidity by gases when

absorbed by actual liquids or by soft colloids," and he shows that those gases penetrate rubber most readily which are easily liquefied by pressure, the rates of passage of carbonic acid and hydrogen being 11·81 and 4·73 respectively; that gases undergo liquefaction when absorbed by liquids and such colloid substances as india-rubber; and finally, that the complete suspension of the gaseous function during the transit through india-rubber cannot be kept too much in view.

Probably the subject with which Mr. Graham's name will always be specially connected is the diffusion of gases. The first paper on it was published in the "Quarterly Journal of Science" for 1829. He found that the lighter a gas is the more quickly it diffuses away from an open cylinder, and the experiments led him to believe that the diffusiveness of gases is inversely as some function of their density, apparently the square root of their density. In this long series of researches the molecular aggregation of the septa through which the gases penetrated was always carefully considered, and in dealing with gaseous penetration he clearly saw that orifices of excessive minuteness might be quite impassable by gases of low diffusive power; that in other cases pores of graphite could only be permeable by *molecules;* and lastly, as will be subsequently shown, that there might be an inter-molecular porosity due entirely to dilatation at a high temperature.

In a paper published in 1831 he established the following law of the diffusion of gases:—The diffusion or spontaneous intermixture of two gases in contact is effected by an interchange in position of indefinitely minute volumes of the gases, which volumes are not necessarily of equal magnitude, being in the case of each gas inversely proportional to the square root of the density of that gas. He speaks of diffusion being effected by a force of the highest intensity, and urges that sensible masses are not affected by diffusion, but only molecules. Diffusion afforded, in Graham's hands, the most conclusive proofs of molecular movement, and as an instance of the rapidity of such molecular mobility it should be remembered that Maxwell has estimated that the initial velocity of a molecule of hydrogen is nearly 1860 metres per second.

The continuation of the researches led him to study the passage of gases through a minute orifice in a thin disc of platinum, a mode of passage, termed *effusion*, which left no doubt of the truth of a general law, that different gases pass through minute orifices in times which are as the square roots of their respective densities, or with velocities which are inversely as the square roots of their respective densities. The research afforded an experimental verification of the truth of the mechanical law, that the velocity with which a gas rushes into a vacuum, through a minute aperture, is the same as that which a heavy body would acquire in falling from the height of an atmosphere composed of

the gas in question of uniform density throughout. Mr. Graham carefully points out that the phenomena of diffusion and effusion are essentially different, although the rates of passage are alike; *diffusion* only permits molecules to pass, but masses pass by effusion.

He found that if the orifice in the platinum disc became tubular, the passage of carbonic acid and nitrous acid became quicker in relation to air than when the rates of passage through a thin punctured disc were examined, and from facts such as this a new kind of passage was detected, which he called transpiration. By employing capillary tubes the effusion and transpiration rates of passage were found to differ widely, "for if the length of the tube is progressively increased, and the passage of all gases becomes greatly slower, the velocities of passage of the different gases are found to diverge greatly from their effusion rates." The velocities at last, however, attain a particular ratio with a given length of tube and resistance, and preserve the same relation to each other with greater lengths of tube and resistances; the most simple result being probably that of hydrogen, which has exactly double the transpiration rate of nitrogen, the relation of these gases as to density being as 1 : 14. Mr. Graham viewed transpirability as being a kind of elasticity, depending upon the absolute quantity of heat which different gases contain under the same volume, and it is therefore more immediately connected with specific heat than with any other property of gases. One very important branch of Mr. Graham's labours appear to have commenced in 1829, with a notice of the "Singular Inflation of a Bladder," which was two-thirds filled with coal-gas, and on being introduced into carbonic acid for some hours became distended. Mr. Graham pointed out that, although Dutrochet would probably view in the experiment the discovery of endosmose acting upon aëriform matter as he had observed it to act upon bodies in a liquid state, the penetration of the carbonic acid was really preceded by an actual absorption in the water with which the pores of the bladder were filled. He continued the research at intervals and published a series of papers a few years before his death, in which he showed that the penetration of gases through colloid septa, such as india-rubber, was the result of an actual occlusion or absorption. A comparison of the relative rates of the penetration of oxygen and nitrogen through india-rubber led to a most remarkable experiment. Oxygen penetrates 2½ times as fast as nitrogen; therefore by dialysing air Mr. Graham actually increased the quantity of oxygen from 20·8 to 41 per cent, just as he had effected a partial separation of oxygen from air by the slightly greater diffusion velocity of nitrogen. The phenomena of diffusion and penetration of india-rubber are, however, widely different, for the rate of passage through the colloid depends on the facility with which the gas is capable of being liquefied within its pores. MM.

Deville and Troost discovered that certain gases penetrated tubes of iron and platinum, and this fact led Mr. Graham to enquire whether the penetration was not preceded by an absorption of the gas. His results proved that palladium, platinum, and iron did absorb hydrogen. Palladium, for instance, absorbs nearly 1000 times its volume of the gas, and most metals appear to have the power of selecting one or more gases in virtue of their colloidal character; for crystalline metals, such as osmium and iridium, do not occlude gas at all. The absorption of hydrogen by palladium led him to conclude that the gas was condensed into the metallic state; but, although much evidence was adduced, it may be doubted whether the metallic character of hydrogen was established.

Under the second classification of the papers, the research on "Arseniates, Phosphates, and Modifications of Phosphoric Acid," occupies a prominent place. It commenced with the explanatory hypothesis that phosphoric acid is disposed to unite with 3 atoms of base, and he then traced the method by which the metaphosphates and pyrophosphates were produced. It is impossible to overrate the importance of this research, for on it the theory of the polybasity of acids mainly rested.

The diffusion of gases appears to have led to the study of the diffusion of liquids, and in 1849 the splendid monograph on the latter subject was published, the results of which showed that diffusion supplied the densities of a "new kind of molecules," for in liquid diffusion we appear to deal no longer with chemical equivalents or Daltonian atoms, but with masses even more simply related as to weight. By continuing the investigation he was enabled to divide various soluble substances into crystalloids and colloids, the former having a rapid diffusion rate and the latter being marked by low diffusibility. He pointed out that, although chemically inert, in the ordinary sense, colloids possess a compensating activity of their own, arising out of their physical properties. The colloid is, in fact, the dynamic state of matter, the crystalloid being the statical condition. The colloid possesses *energy*, and it may be looked upon as the primary source of the force appearing in the phenomena of vitality.

His paper on "Speculative Ideas concerning the Constitution of Matter" will always be viewed with special interest, as it contained several remarkable expressions *of belief*, such as the suggestion that the various kinds of matter recognised as different elementary substances may possess one and the same ultimate or atomic molecule existing in different conditions of movement. With the atom at rest, the uniformity of matter would be perfect: but it always possesses motion due to a primordial impulse, and, as differences in the amount of this motion occasion differences of volume, matter only differs by being lighter or denser matter. The gaseous molecule is composed of a group of the preceding inferior atoms, following similar laws, and is thus

a reproduction of the inferior atom on a higher scale. Chemical combination consists in equal volumes of the different forms of matter coalescing and forming a new atomic molecule, and is therefore directly an affair of weight; and the combining weights differ because the densities, atomic and molecular, differ. And he points out that liquefaction or solidification may not therefore involve the suppression of the atomic or molecular movement, but only the restriction of its range.

All Mr. Graham's results were obtained by the simplest possible means, and it is not a little remarkable that throughout the forty-six papers which the volume contains but few plates and woodcuts are necessary for their complete illustration. All his apparatus which could be found was exhibited at the Loan Collection of Apparatus held during the past year at South Kensington Museum, where it excited much interest from its simplicity as compared with the complicated appliances there gathered together. Some remarks which concluded a description of it may not be out of place here, for it taught that although in certain researches, or for accurate observation and measurement, delicate and complicated instruments may be necessary, the simplest appliances in the hands of a man of genius may yield the most important results. Thus with a glass tube and a plug of plaster-of-paris Mr. Graham discovered and verified the law of diffusion of gases. With a tobacco-pipe he proved indisputably that air is a mechanical mixture of its constituent gases. With a tambourine and a basin of water he divided bodies into crystalloids and colloids, and obtained rock crystal and red oxide of iron soluble in water. By the expansion of a palladium wire he did much to prove that hydrogen is a white metal. And, finally, with a child's india-rubber balloon filled with carbonic acid he separated oxygen from air, and established points the importance of which from a physiological point of view it is impossible to overrate.

Dr. Smith has devoted 29 pages of the volume to a very accurate and valuable analysis of the papers, which he has in many cases collected from scientific periodicals now almost inaccessible even in this country. He has also added an excellent preface entitled "Graham and Other Atomists," which it may be well to quote at some length.

"Atoms and eternal motion are among the first-known scientific ideas. We find them discussed with full keenness by the earliest Greeks of whom we have received definite accounts." Dr. Smith then states the views which have been held from the time of Leucippus, "to whom the action of the atom as one substance, taking various forms by combinations unlimited, was enough to account for all the phenomena of the world." Graham took a similar view, and advanced the idea (of atomic motion with unity of material) to its utmost limit. The Greek told us all was motion; Graham considered that the diversity in motion was only the basis of the diversity of the material, or, in other words, that an atom constituted an element of a special kind ac-

cording to its rate or the peculiarity of its movements. Lucretius, as M. A. Ditte recently observed,* "a exposée la theorie atomique avec une vivacité, un éclat, une éloquence qui font du poëme de la nature un des chefs-d'œuvre de la littérature latine;" and Dr. Smith considers that this philosopher was the only full expositor of the doctrine among the ancients. Newton's well-known words are quoted, as they form an era in the theory of atoms, not so much by new ideas as by distinctness, and because they eliminate much confusion. Newton held that the primitive particles of which matter is composed are incomparably hard and incapable of wear, for otherwise water and earth composed of old worn particles would not be of the same nature and texture now with water and earth composed of entire particles in the beginning, and therefore, that nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations and motions of these permanent particles. The first distinct attempts to define precisely the motion of gaseous molecules, and not of atoms, are by D. Bernoulli, who says—"The chief peculiarities of fluids are these: 1st, they are heavy; 2nd, they expand in all directions unless they are confined; and 3rd, they allow themselves to be compressed more and more, according to the increased force employed." Speaking of a vessel of air with a weighted cover, he says—"So the minute bodies, whilst they impinge on the cover, keep it up by their continually-repeated strokes, &c." The views of Davy, Rumford, and Herapath are then given, and it is pointed out that Joule investigated experimentally the hypothesis that gases consist of small bodies continually impinging on one another, their elasticity increasing with the temperature, the pressure of the gas being due to the impact of the particles against any surface presented to them. Dr. Smith shows that it was the object of Graham's life to find out what the motion of the atom was, and claims him to be "as strict an atomist as perhaps can be found," and it is certain that he did believe in the existence of atoms, but few minds have been more free from unconscious bias: we are convinced that he would have abandoned the atomic theory without reluctance if he had found a better to replace it.

We may quote, as it will be familiar to most readers, a paragraph just written by Mr. Gladstone,† which will aid us in explaining Mr. Graham's mental attitude. The learned writer points out that "even in the days of Bacon, even in the days of Dante, when knowledge, as the word was commonly understood, was so limited that some elect minds of uncommon capacity and

* "Constitution de la Matière," par M. Alfred Ditte. Ann. Chim. et Phys., Feb., 1877, p. 151.

† On the Influence of Authority in Matters of Opinion. By the Rt. Hon. W. E. Gladstone, M.P.

vigour could grasp the whole mass of it, they still depended largely upon authority. For that aggregate of knowledge, which they were able to grasp, was but book knowledge and not source knowledge." It is hardly too much to say that Mr. Graham had grasped, early in his career, all that was then known of chemistry and physics. He cared but little for the prevailing theories of his time, but for all "source knowledge" derived from experiment he had the highest respect. In his first paper (in 1826) he alludes to the researches of "that ingenious chemist Mr. Faraday," and he constantly quotes the labours of others or makes their work a starting-point for fresh discoveries. No man ever had a higher reverence for authority or more constantly appealed to it.

SCIENTIFIC NOTES.

GEOLOGY, MINING, MINERALOGY, AND METALLURGY. — We have from time to time felt it our duty to draw attention to the services rendered to science by the American Government by means of the admirable Geological Survey of the Territories now being carried on at its expense and under its direction. It is therefore the more satisfactory that the British Empire is not behind the labours of a similar class. The Geological Survey of India may not include any notice of the living flora and fauna of that country, so rich in both departments of organic life, but the account of the fossil remains is given on a most magnificent scale. This will be at once apparent, when we mention that the issues before us, comprehending sixty large and well-executed plates, with the accompanying descriptions, is entirely devoted to one nook of the country, the province of Kutch, and to one group of Mollusca, the Cephalopoda, formerly known as ammonites. The value of documents, so full and so trustworthy, cannot be easily over-estimated. Our readers will regret to learn that Dr. Waagen, on whom has devolved the task of determining, classifying, and describing the specimens discovered, has been compelled, by ill-health, to give in his resignation, and return to Europe.

Dr. E. A. Smith's "Report of Progress for 1875 of the Geological Survey of Alabama" contains a general outline of the geological formation in the State, a history of coal-mining in Alabama since 1853, with notices of the features of the fields, of the character of the coals, and of the fossil vegetation. Copper ore exists in the metamorphic region, and is worked to a small extent. The ores are yellow sulphide, averaging some 10 per cent. of metal, also azurite and malachite. There is also a preliminary notice on the cotton-worm, the larva of *Aletia argillacea*, a small moth belonging to the Noctuidæ. The use of the arsenical compounds of copper is proposed for its destruction,—a method, as it appears to us, questionable on grounds of public health.

An account of the fossil organic remains discovered during the Geological Survey of the Province of Victoria, and used for the determination of the ages of the different geological formations of the region, is being issued in numbers of ten plates each, with corresponding descriptions, on the plan of the decades of the Geological Surveys of England, Canada, India, &c. The first plate is devoted to the skull of *Thylacoleo carnifex* (Owen). Much interest attaches to this extinct species from the controversies which have been waged concerning its nature and habits. Prof. Owen named it the Marsupial Lion, from the general resemblance of its teeth to those of the lion, indicating, in his opinion, its decidedly predaceous character. Dr. Falconer, Mr. Flower, and others, took a different view, and considered it as a harmless plant-eater, since a præmolar of a sharp-edged compressed form, like the carnassial of *Thylacoleo* is found in the still existing rat-kangaroos (*Hypsi-prymnus*). As Mr. F. McCoy, however, points out, they overlooked the fact that the *Hypsi-prymni* have behind the compressed præmolar a series of grinders of the ordinary vegetarian type, whilst in the marsupial lion all the teeth are of the true carnivorous type. Mr. McCoy considers that his Victorian specimen presents important differences from the New South Wales examples described by Prof. Owen, and that the species of the two provinces are really distinct. He would retain for the Victorian animal the name of *Thylacoleo carnifex*, and proposes that of *T. Oweni* for the New South Wales species.

The third report, since the re-modelling of the Mining Department of he Geological Survey of Victoria, has been issued. It forms a quarto

volume of over three hundred pages, more than a third of which is taken up by Mr. Brough Smyth's report on the progress made in the Geological Survey of Victoria during the year ending September 30th, 1875. Since the reorganisation of the department nearly 10,000 square miles of country have been surveyed and mapped on different scales, varying from one-eighth of an inch to 2 inches per mile, the latter being the scale of the Geological Survey, a much more convenient scale, by the way, for working with than our own. Besides this, nearly 3000 square miles are being surveyed and mapped on scales varying from half an inch to 2 inches to the mile. Perhaps the most important work done during the year is the publication of a new and corrected edition of the Geographical Scotch Map of Victoria on the scale of sixteen miles to the inch, representing all that is known of the geology of that country, and a first sketch of a geological map of the whole of Australia on the scale of 110 miles to the inch, in which are given the results of all explorations respecting which information is available, from the first discovery of the Continent up to the present time.

Mr. Krause's "Report on the Ararat Gold Field" gives the history of the discovery and progress of one of the most celebrated of our modern El Dorados, his information being derived from the *vivâ voce* accounts of the few surviving pioneers of this gold field. The first "rush" was made in 1855, and before many months a town built of canvas, wood, and even stone, sprang up, which gave shelter to upwards of 3,000 souls. A bye-road leads past the place to-day, and nothing remains to indicate the former bustling crowd but a few ruined stone huts, themselves almost hidden by a thick forest of young gum saplings sixteen or seventeen years old.

Mr. Reginald Murray's "Report on South Western Gippsland" shows that he has completed a Geological Sketch Map of the district, which extends over 3500 square miles. Mr. Howitt has done the same for North Gippsland, and the two reports form a most valuable addition to our knowledge of the geological structure and mineral resources of this interesting portion of the Australian Continent. Other reports follow on the Stawell Goldfields and the Kilcunda and Patterson Coalfields.

The question of the existence of gold in solution in the saline waters of mines has received a large amount of attention from the chemists of the Department. The employment of native pyrites as a source of sulphur in the manufacture of sulphuric acid has also received a large amount of attention, but as nearly all the pyrites found in Victoria contain arsenic in notable proportions, the Melbourne sulphuric acid manufacturers prefer to import native sulphur. The part played by particles of organic matter in the formation of nuggets has also received attention. A solution of gold terchloride containing a piece of rough metallic gold was found by Mr. Daintree, one of the chemists of the Department, to have become perfectly colourless. On further examination a piece of cork was found floating on the surface, and the piece of gold had so much increased in size that it would no longer pass through the neck of the bottle. The experiment was repeated with pieces of hammered gold, without success, but whenever the surface of the metal was roughened, deposition always took place.

Judging from the "Mineral Statistics of Victoria" for 1875, Nos. 11 and 12, alluvial gold mining has long ago seen its last days in Victoria, having fallen from a million ounces in 1868 to a little over 400,000 ounces in 1875. Quartz gold mining seems to be also on the decline, there being a steady decrease of 50,000 ounces between the returns of 1872 and 1875, the falling off from the previous year being over 22,000 ounces. The number of miners, too, is diminishing, having fallen from 58,000 in 1871 to 41,000 in 1875, the alluvial miners decreasing the more rapidly of the two. The Chinese miners are following suit, but this may be regarded as a blessing. The average earning of each man, however, is on the increase, the figures in 1875 being £104 as compared with £93 in 1871, showing that improvements have taken place in the mode of working. The deepest quartz mine is at Pleasant Creek, the lowest working being 1706 feet below the surface, proving that gold quartz can be worked

profitably at great depths. The total quantity of gold exported and minted in the colony from the date of the first d scovery to December 31st, 1875 is 45½ millions of ounces, valued at 182 millions of pounds sterling. Tin ranks next for £333,870, after which comes antimony for £104,865. Silver only shows for £14,617, copper for £8331, lead for £4700, iron for £2101, and coal for £8233, for the whole period to the end of 1875, showing that the mineral wealth of Victoria is to be found almost entirely in its gold-mines. Not a single ton of coal was either raised or exported during the year 1875, the imports of that article amounting to 214,000 tons.

"The Reports of the Mining Surveyors and Registrars for the Quarter ending March 31st, 1876," give the gold-mining statistics of the colony for the first quarter of the present year, and show the yield of gold from parcels of quartz raised during the quarter from some of the deepest mines in Victoria, the depth of the shaft &c., the estimated yield and quantity extracted, the gold received at and issued from the Royal Mint, the yield of gold from quartz and tailings, as well as from washing operations, and the number and distribution of miners on the gold-fields. The coin issued from the Melbourne Board of the Royal Mint during the quarter has been 124,046·53 ozs., besides 1,100·914 ozs. of gold bullion.

We have received an interesting little *brochure* on the minerals of New South Wales from the pen of Prof. Liversidge, who holds the chair of mineralogy in the University of Sydney. The author gives additional description of the minerals found in New South Wales, with their chemical composition and the localities in which they are found. In the account of the different gold-fields an important statement is made with respect to the first discovery of gold in Australia, which is worthy of record. It seems that, according to the "Evening News," of Sydney, for August 7th, 1875, that gold was discovered and officially reported to the Government of the day on February 16th, 1823, by the Assistant Surveyor, James McBrian, at a spot on the Fish River, about fifteen miles east of Bathurst. Mr. McBrian's field book is preserved in the Surveyor General's office, and contains the following entry:—"February 16th, 1823. At 81·50 to river and marked gum tree. At this place I found numerous particles of gold in the sand and on the hills convenient to the river." The use of the word "convenient" in conjunction with the worthy Assistant-Surveyor's patronymic seems to show that the first discoverer of gold in Australia was an Irishman. The knowledge of the discovery seems to have been suppressed, owing to the peculiar social condition of the colony at the time. The same thing was done when Count Stozelecki, in 1839, and the Rev. W. B. Clark exhibited specimens of gold to members of the Legislature of those periods.

Mr. Frederick Field, F.R.S., thus describes a Cornish mineral which is quite new to mineralogical science. "Its crystallisation appears to be rhombic; it is transparent and brilliant, of a clear green colour; hardness about 3·5. It is perfectly soluble in dilute hydrochloric acid, forming a nearly colourless solution. On heating a little over 100° C., the crystals lose water, and at a low red heat at once become brilliantly black. They decrepitate strongly. Analysis proved the existence of phosphoric acid, ferrous oxide, and water in the proportions that would lead to the formula $3FeO,P_2O_5,4H_2O$. From the great scarcity of the mineral only small quantities can be obtained for analysis, and this brief description must be regarded merely as a preliminary note on a mineral of great beauty and interest."

A meteoric iron, very rich in nickel, found in the province of Santa Catarina (Brazil) has been analysed by MM. Guignet and G. Ozorio de Almeida. The specimen contained 36 per cent of nickel, and is free from chrome, cobalt, manganese, and copper; neither is it mixed with any earthy gangue.

In a paper on Russian platinum-ore from the Oural Mountains, Mr. Sergius Kern, of St. Petersburg, says that considerable quantities of platinum-ore are every year mined in Nishni-Tagil and Goroblagodatsky districts on the Oural Mountains. The platinum-ore of these districts contain notable quantities of

oreign metals of the platinum group, except ruthenium, which is only in traces found in these ores. He also gives analyses of some newly found platinum-ores near these districts. which may be of some interest to chemists studying the properties of the compounds of rare metals of the platinum group as the ore is sold at the Mint in St. Petersburg at a very moderate price.

Microscopy.—The oxyhydrogen microscope has been greatly improved by Mr. John Browning. Those who are familiar with the old instrument will notice a great increase of defining power, and when a suitable surface is employed to receive the image, some of the coarser diatoms, such as *Arachnoidiscus* and *Isthmia* are not only fairly shown, but will bear being viewed with some amount of additional magnifying power. Great care has been taken in the arrangement of the condenser to prevent the object from being injured by the intense heat of the lime-light, and, with such success, that balsam-mounted slides can be used with safety. The object-holder is adapted to receive the ordinary 3×1 slide. This instrument will prove a great desideratum for lecture room and class demonstration, and for this purpose, when the object is not too opaque or of an unfavourable colour, and tolerably flat, it will be found all that could be wished. It works well upon a disc of from 4 to 5 feet in diameter.* Microscopists must not expect even of this improved instrument the resolving or defining power they have been accustomed to with their usual microscopes; the conditions are entirely different, and involve many constructive difficulties, and the lime-light is, after all, a feeble source of illumination where great magnifying power is required. The instrument is rather to be judged in comparison with the older forms of gas microscope, and, when so examined, will be found a great improvement on all its predecesrors.

The President of the Royal Microscopical Society, H. C. Sorby, Esq., F.R.S., in his Anniversary Address laid before the Society his observations on the structure of disintegrated rocks, such as occur in various sands, clays, &c. He was led to investigate the subject chiefiy by having undertaken to examine and describe the mineral constituents of the deep ocean deposits brought back by the "Challenger." For the results of these researches our readers must be referred to the original paper † as the subject cannot well be abridged; as, however, the processes employed in investigation may aid other observers, they are given at some length. When stratified rocks are sufficiently hard and consolidated to be made into thin and partially transparent sections, many facts may be better seen in slices cut perpendicular to the stratification than by attempting to disintegrate the rock and examine the detached particles. It would, however, often be difficult to prepare thin sections of many deposits, and it becomes necessary to study them in another manner. If the particles are firmly held together by calcic or ferrous carbonate, or by any of the oxides of iron, they may be set free by the action of cold dilute hydrochloric acid, or by a stronger hot solution: but if the rock is consolidated by means of silex, this cannot be accomplished. Violent mechanical crushing must be avoided, since it would give rise to false results by fracturing the constituent grains. Such an amount of crushing as can be effected with a small stiff brush made with bristles does, however, appear to be admissible, since it could scarcely break the separate fragments. A portion of the crushed material may then be sufficiently diluted with more water, placed on a glass slip with a projecting ledge, and covered with a piece of the usual thin glass. By this means the larger particles are seen separate, but the smaller have a great tendency to mass themselves together. Since the index of refraction of the various grains is in all cases considerably greater than water, the outline of even the most transparent constituents is well seen, but at the same time this difference in refractive power may make it impossible to study the internal structure or optical characters of the larger grains. This difficulty is overcome by mounting in Canada balsam, which has so nearly the same index of refraction as that of many of the constituent grains that, even when

* With a powerful electric light a disc of 7 or 8 feet diameter can be obtained.
† Monthly Microscopical Journal, vol. xvii., p. 113, March, 1877.

their outline is as irregular as possible, light passes through them almost as though they were thin slices with parallel polished surfaces. This enables us to study the external staining, internal structure, and optical characters to great advantage, since they are not interfered with by any dark shading due to the bending of the light out of the line of vision. When examined in water there is no difficulty in recognising extremely minute granules of the kaolin of clays, whereas, when mounted in balsam, they may be almost or quite invisible, but this circumstance is of great advantage in observing certain facts, since, by making them invisible, other objects may be distictly seen which otherwise would be completely hidden by the surrounding granules.

Mr. Sorby mounts loose sandy deposits in the following manner:—Having placed a very small quantity of dissolved gum on the glass plate, the requisite amount of the deposit is taken and mixed with the gum, and sufficient water to make it easy to separate the grains and spread them uniformly over the space, which will be afterwards covered with the thin glass. The water is then allowed to evaporate slowly, and though much of the gum collects round the margin, by properly regulating the quantity originally added, enough remains under the larger grains to hold them so fast that they are not squeezed out with the excess of balsam. More gum than is sufficient for this purpose should not be used, since it may make itself too conspicuous on the object. When the proper quantity has been used its presence can be detected only at the under-surface of the grains, and in that situation does not in any way interfere with the study of the object. This method prevents the grains from settling to one side of the object, even when soft balsam is used, which is desirable, since it penetrates more completely at a lower temperature into irregularities of the surface, and into the interior of compound grains. In these observations, the employment of object glasses of large angular aperture is disadvantageous, and with the higher powers it is impossible to focus down into the interior of grains of sand so as to view their minute fluid cavities. For these researches Mr. Sorby has had an eighth of only 75° of aperture constructed by Messrs. Beck, with which all parts of the object can easily be reached and perfect definition obtained with a power of about 600 linear; the form of grains as small as $\frac{1}{5000}$ of an inch in diameter can also be seen. In the case of very fine transparent particles of such substances as pumice, the form is best seen when the illuminating pencil is rendered divergent by means of a conclave lens, instead of employing the usual condenser. This mode is, however, only available for the lower powers, on account of the light being somewhat feeble. A considerable portion of the paper is occupied with detailed information respecting the examination of various fragmentary rocks and the means employed for their identification. Considerable use is made of the polariscope, as in the examination of rock sections. The paper is full of matter of the utmost value to the micro-mineralogist and geologist.

Light.—M. Bert has undertaken some new experiments upon the influence of different colours upon vegetation. These experiments, performed chiefly upon the sensitive plant, lead to the following results:—Green light kills plants; plants submitted to the influence of the green ray die in a short time. Under the influence of the red rays the sprays become elongated; the leaflets are raised so as to form a smaller angle with the branch than in the normal state; the plant appears to become etiolated and yet it remains alive. Under the influence of the blue rays the process is reversed, the leaflets become perpendicular to the branch, whilst in white light an intermediate position is maintained, *i.e.*, the leaflets form with the branch an angle of 45° on one side and of 75° on the other. M. Bert explains these facts as follows:—At the level of the point of attachment of the leaflet there is a motor enlargement, which increases or lessens in force according to the different kinds of rays. Under the influence of red rays there is formed in these enlargements a particular substance, osmotic, and capable of attracting water. This substance generally disappears under the influence of blue rays. If we place it under a glass shade, red on one side and green on the other, the plant turns its leaflets towards the green, that is to say, towards the colour which kills it, and in fact it dies.

Electricity.—A new electric lamp designed by M. P. Jabloschkoff has been described to the Academie des Sciences by M. L. Denayrouse. The new source of light is composed of two pieces of charcoal fixed in a parallel position at a little distance from each other, and separated by an insulating substance capable of wasting away at the same speed as the charcoal. When the electric current begins to pass the voltaic arc is formed between the two uncovered extremities of the two charcoals. The nearest layer of the insulating matter melts, is volatilised, and slowly lays bear the two rods of charcoal just as the wax of a candle progressively uncovers its wick as the combustion is propagated downwards. The heat springing from the combustion of the charcoal is utilised for the fusion and volatilisation of the insulating mixture. The composition of this latter may be varied indefinitely, since most earthy matters may be employed. The simplest mixture provisionally adopted consists of sand and powdered glass, which, with an equal electric power, gives double the light of a regulator. The author has been able to divide the light produced by a single source of the current. With a single Gramme machine of the common make he has caused three sets of charcoals to burn at once.

We learn from the "Polytechnic Review" that, in order that ordinary fireproof safes should be in a measure rendered burglar proof, the Louis Reutzsch Manufacturing Co., of Meissen, has constructed a wire covering, which is placed in an electrical circuit with an alarm bell. If any of the wires forming the cover be cut, which must necessarily happen before the safe can be opened by an intruder, the circuit is broken and the alarm bell is sounded. The device is likewise applicable to doors and windows.

Mr. Alexander Bain, the inventor of the electro-chemical telegraph, died in January last in the Home for Incurables, near Kirkintilloch, of which institution he had been an inmate for several years.

Mr. Alfred Smee, F.R.S., &c., the inventor of Smee's battery, also died in January last. Mr. Smee was the author of "Elements of Electro-Metallurgy," "Elements of Electro-Biology," and several other scientific works.

Technology.—Oenokrine is the name of a test-paper sold in Paris for the purpose of detecting the fraudulent colouration of wines. With a genuine red wine the colour produced is a greyish blue, which becomes lead-coloured on drying. With magenta and other aniline colours it turns a carmine-red; with ammoniacal cochineal, a pale violet; with elder berries, the petals of roses, &c., a green; with logwood and Brazil wood, the colour of dregs of wine; with Fernambucca wood and phytolacca, a dirty yellow; with extract of indigo, a deep blue. The manipulation required is very simple. A slip of the paper is steeped in pure wine for about five seconds, briskly shaken in order to remove the excess of liquid, and then placed on a sheet of white paper to serve as a standard. A second slip of the test-paper is then steeped in the suspected wine in the same manner and laid beside the former. It is asserted that 1-100,000th of magenta is sufficient to give the paper a violet shade, whilst a larger quantity produces a carmine-red. The inventors of the test-paper, MM. Lainville and Roy, are also said to have discovered a method of removing magenta from wines without injuring their quality, a fact of some importance if it be true that several hundred thousand hectolitres of wine sophisticated with magenta are in the hands of merchants.

In a letter written from Mentone to the Manchester Literary and Philosophical Society, Mr. Joseph Sidebotham, F.R.A.S., says:—"My attention has been for some time directed to the growing use of the aniline colours for tinting photographs. Now I find they are being *extensively* used in paintings and water-colour drawings, and the colours regularly sold for that purpose. Anyone who knows the speedy alteration by light of nearly all of these colours will protest against their use, and a statement of this with the authority of some of our chemists would probably have the effect of causing them to be discontinued by all artists who care to think that their works should last more than a single year."

Mr. A. F. Taylor, of Andover, Mass., sends the following note on poisonous india-rubber toys to the "Boston Journal of Chemistry:"—"Prof. B. Tollens, in the "Journal of the Berlin Chemical Society," of November 13, 1876, calls attention to the injuriousness of many of the articles manufactured from caoutchouc, which, among other impurities, contains a very large per cent of zinc oxide. In the rubber nipples of milk-bottles for children this has often been found to be the case, and so much attention has been called to this fact that the manufacture of these nipples containing zinc oxide has to a great extent ceased. But more recently suspicions have been aroused concerning the quality of children's toys, dolls, animals, &c., made from rubber. One case, in which a child, having one of these dolls, had had it for some time in its mouth, grew sick, and the doll, laid in vinegar, became covered with an incrustation (without doubt zinc acetate), led to direct investigation. In 0·7325 grm. of such a doll, 0·4446 grm. zinc oxide was found, or 60·58 per cent. Another portion gave, after being subjected to a red-heat, 62·64 grms. of ash, yellow while hot, white on cooling. In the ash besides the zinc were traces of lime, iron, and phosphoric acid. From another doll which had been warranted 'harmless' 57·68 per cent of ash was obtained, consisting almost wholly of zinc oxide."

The following gentlemen were requested by the Council of the Royal Dublin Society to consider and report upon the scientific prospects of that Society:—Sir Richard Griffith, Bart., F.R.SS. L. and E.; H. Lloyd, D.D., F.R.SS., L. and E., Earl of Rosse, F.R.S.; M. H. Close, M.A.; W. F. Barrett, F.R.S.E.; W. R. M'Nab, M.D.; Gerald Molloy, D.D.; Alexander Carte, M.D.; Alexander M'Donnell, M.A., J. Emerson Reynolds, M.D., F.C.S.; Wm. Andrews; Edward Hull, M.A., F.R.S.; Alex. Macalister, M.D.; B. B. Stoney, M.A., M.I.C.E.; Lord Gough, Howard Grubb, M.E., F.R.A.S.; Robert S. Ball, LL.D., F.R.S.; Charles A. Cameron, M.D.; Robert M'Donnell, M.D., F.R.S.; G. Johnstone Stoney, M.A., F.R.S.; Charles Kelly, M.A., Q.C. From their report we learn that, while while the Royal Dublin Society during the century and a half of its existence has engaged in many most useful branches of scientific work, its labours in relation to some of these have been from time to time superseded owing to their being entrusted by the Government to Public Departments, in order to continue them on a larger scale than the Society could have attempted; and the Society's work has been thus from time to time circumscribed by the establishment of such institutions as the Ordnance Survey of Ireland, the Geological Survey of Ireland, and the Royal College of Science. It is now proposed that the Society's prolonged scientific labours in connexion with its Museums and Botanical Garden shall in like manner be undertaken by the Government. If the Royal Dublin Society assent to this change the Committee are of opinion that it will do a very important service both to science and to Ireland if it can mould itself into two societies, of which one shall be devoted to science, pure and applied, in all its branches, and the other to agriculture. It is recommended that the present members of the Royal Dublin Society be members of both these Societies, and that those existing members of the Royal Irish Academy who are not already members of the Royal Dublin Society shall have privileges with respect to the Scientific Society corresponding with those to be enjoyed by members of the Royal Dublin Society. It is further recommended that within the Scientific Society there be an inner body of Fellows.

THE QUARTERLY

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I. THE CHEMISTRY OF THE FUTURE.

IN selecting this subject we lay no claim either to the gift of second sight or to the possession of a clearer view of the future development of Science than may be the lot of our contemporaries. Nor is it our purpose to write an imitation of Winterl's celebrated "Prodromus," in which enthusiasts may find whatever they think proper. We seek merely to call the attention of our fellow-workers, and especially of students, to certain researches which hold out great promise, and which, if duly followed up, will undoubtedly have a most important influence on the very foundations of chemistry.

It must be confessed that, as regards these very foundations, the alphabet of the science, our knowledge is not merely limited, but unsatisfactory in the highest degree. Look at our "elements." Most chemists quietly accept them as ultimate facts, and work with them—or perhaps play with them—more or less judiciously, quietly waiving all inquiry into their nature and their origin. Are they absolutely elementary bodies, distinct from the beginning, and resolvable neither into each other nor into any forms of matter still unknown? Or are they compounds, elementary in the mere relative acceptation that their decomposition is a task not within our present knowledge and power. Have we any evidence of their simplicity other than what our grandfathers had of the supposed elementary character of potash and soda prior to Davy's great discovery? If compounds, are they all of the same order, or are some of them, perhaps, resolvable into the remainder, whilst these, in turn, consist of ultimate—or at least ulterior—bodies, as yet undiscovered? If simple, are they like the wheels and pinions

of a machine, or the parts of a dissected puzzle, definite in their number and purpose, and all necessary to a given result? Or are they a mere fragmentary and accidental group of objects with which we build our *Lagerungs formel*, just as children construct their sand forts and shell grottoes on the shore? What would be the impression of a man of inquiring mind if for the first time made acquainted with the so-called elements and their leading properties? He would see a list of some sixty bodies, from which, he is told, all things visible or tangible—all matter, in short—are compounded. But why their number should be between 60 and 70, rather than between 30 and 35 or between 160 and 170, no reason is given or even conjectured. Some of them, he is informed,—such as oxygen, hydrogen, nitrogen, silicon, aluminium, and sulphur,—are exceedingly abundant. Others, on the contrary,—as vanadium, thallium, indium, cæsium, and gallium, occur only in minute traces. Some are widely distributed, and others concentrated in comparatively few localities. Turning to their properties he finds equal difficulties. Here he will see a number of "elements" identical, or at least closely approaching, in their atomic weights. There, on the contrary, he finds wide gaps. Thus betweeen cerium (140) and erbium (178) there intervenes not an element. A smaller blank is found between bismuth (208) and thorium (231), between tungsten (184) and osmium (195), between zinc (65) and arsenic (75); whilst, on the contrary, between 86 and 96 we number six elements, and between 195 and 200 five (see Table I.). These gaps may, indeed, be possibly filled up by the discovery of some rare element, but there is also the possibility that new discoveries may fall in the more thickly-filled parts of the series. With those properties of the elements which cannot as yet be exhibited in a numerical series the case is very similar. There are groups showing a close approximation in their characters and behaviour. There are elements which stand comparatively isolated.

How are all these facts to be explained on the theory of elements primordially distinct? Popular opinion, here as elsewhere, takes refuge in teleology. The elements in their respective proportions and in their distribution exist, as we find them, for the sake of man's convenience. We demur to this hypothesis. Look at sulphur; where existing in quantity the very key with which we unlock the treasure-house of Nature—no less essential to the chemical technologist than is iron to the engineer; but where occurring in small quantities, what a source of evil! Take the case of

coal: there is probably no purpose to which we apply it—in metallurgy, in the gas-manufacture, in the generation of steam, or in domestic economy—where its value is not seriously impaired, and where nuisance and danger are not occasioned by the presence of a small percentage of sulphur. Far too little and far too equally distributed to be worth, or even capable of, extraction, it is yet far too much to be inactive, or other than formidable. Or, again, take phosphorus: absolutely necessary to our existence, and, of course, most valuable when met with in quantities and forms capable of utilisation; but when found, as it so frequently is, in traces accompanying iron, it is the source of incalculable loss and annoyance. Or, consider arsenic: on teleological principles, surely, a substance so poisonous, and yet at the same time so useful for certain technological purposes, should have been concentrated in some few places. But we find it very widely disseminated, present to a serious extent in most iron pyrites, and thus contaminating sulphuric and hydrochloric acids, and through them a variety of other chemicals.

On the other hand, we look at the case of gold: had it been more abundant, its low affinity for oxygen and sulphur and its power of resisting the action of organic acids would have rendered it exceedingly important, both for manufacturing and domestic purposes. Were gold as plentiful as copper the latter metal would be entirely banished from dye- and print-works.

As for the rarer metals, if they exert any function it escapes, as a rule, our notice, whilst not a few of them, if common, would have been highly important. Thus on teleological principles we see nothing to explain, either the number, the relative amount, or the local distribution of the elements of many of them, scarcely, even, the very existence.

We turn, therefore, to another point of view. The array of the elements cannot fail to remind us of the general aspect of the organic world. It shows us the same gaps due to our old acquaintance, the "missing link," or rather to its non-appearance. In both cases we see certain groups well filled up, whilst other forms stand isolated. Both display species that are common and species that are rare. Hence it seems natural, in the one instance as in the other, to view existing forms not as originally present, but as the outcome of a process of evolution, or, if the reader likes the expression, the residue after a "struggle for existence." Certain forms not in harmony with the present general

conditions have disappeared; certain others have maintained themselves, indeed, but only on a limited scale; whilst a third class are abundant because circumstances have been favourable to their formation and preservation. The analogy, it must be remembered, is not the closest, and must not be pushed too far. There is of necessity a wide difference between species composed of lifeless beings, incapable of growth, reproduction, and decay, and species consisting of living organisms. From the nature of the case there cannot occur in the "elements" any distinction corresponding to that between living and fossil organic forms. The "stone book" tells us nothing of extinct elements. Nor would we for a moment suggest that any of our present elements, however rare, is disappearing; that any new element is in the course of formation; or that the properties of such as exist are in course of modification. All such changes, in as far as they took place at all, must have been confined to the pre-geological epoch—to the time when our earth, or rather the matter of which it consists, was in a state very different from its present condition. Making, however, every allowance for these distinctions, if evolution is the law of the universe manifested in the heavenly bodies, in organic individuals, and in organic species, we shall probably recognise it also—though under an especial aspect—in those elements from which stars and organisms are, in the last resort, compounded.

But where is the evidence that the elements have been formed by the "expansion" * of some few antecedent principles, at present hidden, or perhaps from one only primordial kind of matter? Were our attention confined to our own planet we might perhaps find little either to verify or to confute our hypothesis. But if we institute a comparative examination of the chemical composition of different heavenly bodies, as revealed to us by the spectroscope, evidence will be found. Into its nature and value we will now proceed to enquire.

On an examination of the spectrum of our sun we recognise certain elementary bodies as decidedly present in his atmosphere. These are sodium, calcium, barium, magnesium, iron, chrome, nickel, copper, zinc, hydrogen, aluminium, titanium, manganese, and possibly strontium, cadmium, and cobalt. The following terrestrial elements, however, appear to be absent:—Gold, silver, mercury, rubidium, tin, potassium, lead, antimony, arsenic, lithium,

* *See* Quarterly Journal of Science, January, 1877, p. 26.

silicon, glucinum, cerium, lanthanum, didymium, ruthenium, iridium, palladium, and platinum. The remaining terrestrial bodies may possibly still be discovered in the sun, as there are many lines not yet identified. On the other hand, it is quite possible that some of these lines may indicate the presence in the sun of elements other than our own, and possibly of a simpler nature.

The spectra of the stars again differ from those of the sun. Meteorites have not, to our knowledge, been subjected to as careful and thorough spectroscopic examination as they deserve; but on ordinary chemical analysis they are found to contain some of the elements present in the earth, but wanting in the sun, viz., silicon, potassium, tin, and probably antimony, arsenic, and lead.

Thus we see that nineteen well-characterised terrestrial elements are absent in the sun. To explain this fact three hypotheses may be proposed:—We may assume that each sun or planet is a body independent in its origin and materials, and not therefore necessarily containing the same chemical elements as its neighbours. This view is, of course, irreconcilable with the nebular hypothesis, and though it might have met with general acceptance in the beginning of the century, we think it will fail to command the assent of the most judicious physicists of the day.

Or, secondly, we may conceive that the missing elements, from some of their attributes, are more likely to escape observation than are others. It is quite possible that simple bodies present in the sun in exceedingly small proportions —bodies of very high specific gravity or of sparing volatility—might not be recognised in the spectrum; but on going carefully over the list of the missing elements we can scarcely pronounce such considerations admissible. Among these elements we find one, silicon, which on our earth ranks among the most plentiful bodies. We perceive, indeed, certain bodies of a decidedly fixed character,—such as gold, platinum, and iridium,—but along with these occur such signally volatile substances as potassium, mercury, arsenic, and antimony. The heaviest bodies are included, such as gold and platinum, but also lithium and potassium, which are remarkably light. Perhaps we may say that the missing elements have, on the average, higher atomic weights than those bodies found present in the sun.* Yet the former list includes glucinum and lithium, which rank, in this respect,

* This circumstance, we think, favours the hypothesis of the compound nature of our present elements.

among the lowest of the metallic elements. We do not, therefore, see any reason why the above nineteen elements, if present in the sun, should elude our researches.

The third hypothesis seeks to account for the absence of certain terrestrial elements in the solar atmosphere by assuming that at the temperature there existing such elements would be dissociated, or rather that they could never be formed. This supposition may be tested by the following methods :—

a. Physicists conclude that all the fixed stars are by no means equal in the energy of the thermic phenomena which they display. Some emit a pure and intense white light, and seem to be hotter than our sun ; others, of a deep yellow or red hue, are supposed to be in a more advanced stage of their career, and to have greatly cooled down. If this third hypothesis is correct the white stars will contain none of the elements absent in our sun, whilst some of those distinguished in him will in them probably be missing. With the redder—or, generally speaking, the more highly-coloured —stars, on the contrary, some of the elements absent in our sun ought to be recognised, and none which are found in him ought to be wanting. There is also, as we have already hinted, a further consideration : if any of our elements cannot exist in the sun or in the stars, it becomes highly probable that in the solar and stellar spectra there will be indications of bodies, not existing *as such*, on our globe, but which are the materials from which such missing elements are composed. Mr. Lockyer, to whose researches in this direction we shall shortly refer, considers that a series of photographs of stellar spectra will afford valuable information in regard to the constitution of certain substances now regarded as elementary. We may here remark that the discovery, in the sun or in the stars, of any element not found in the earth, is, with the means at our disposal, scarcely conceivable. The spectroscope shows us certain lines : some of these we identify with the characteristic lines of terrestrial elements ; others we fail to identify, and therein we reach, for the present, our limits. How can we, from out of the mass of unidentified lines, select one as characteristic of x, another as belonging to y, and a third as peculiar to z, when these bodies are strictly, to us, unknown quantities ?

b. The next method for the verification of the above hypothesis is a direct attack of some of the supposed elements —perhaps preferably some of the nineteen found wanting in the sun—from the most promising point of view. There may, perhaps, be little hope that any of our present simple

bodies can be successfully assailed by any chemical process "pure simple," that is, by bringing it into reaction with any agent calculated to abstract from it some one of its unknown constituents, setting another (or others) at liberty. Probably an intense temperature would be the most likely means of approaching the conditions existing in the sun. A practical difficulty is here to be encountered or evaded. It is perfectly possible that at extreme temperatures some element might be dissociated,—the result may have been obtained already,—but that on cooling re-combination has taken place, the apparent result being *nil*. Mitscherlich and Plücker have observed, some years ago, that in the hydrogen flame iodine showed broad bands, whilst in the far higher temperature produced by the electric spark it displayed merely bright lines. Hence, if it be correct that bright lines are characteristic of elementary bodies, whilst bands are produced only by compounds, it may be inferred that iodine when heated in the spark becomes dissociated, and resolves itself into some elements not yet detected.

But the substance whose elementary character has been of late most strongly called in question is calcium. Some time ago Mr. Lockyer declared his conviction that it is not a simple substance, but that the H lines in its spectrum are due to two elementary bodies of which it is composed. In the photographs of the spectrum of *α* Lyræ taken by Dr. Huggins only one of these H lines is present—a fact which leads to the inference that only one of the constituents of the metal calcium can be present in the star *α* Lyræ. This point, therefore, requires a most careful examination. It is interesting to find that researches of a totally different character have led an eminent chemist of the present day to entertain grave doubts concerning the homogeneous nature of this same metal. Whether the phenomena observed indicate that calcium is really a compound which under certain circumstances suffers a partial decomposition, or whether they merely prove that under the name of calcium we confound two—or perhaps more—metals, identical in their atomic weights, and closely approximating in their respective properties, as is the case with cobalt and nickel, it would be premature to decide. Nor can we anticipate the gentleman concerned by indicating the class of reactions which he is now submitting to a rigorous examination. Should these suspicions be confirmed the result will be in either case interesting, and may probably throw an unexpected light upon certain questions in geology, mineralogy, and physiology. Still, in the second alternative, instead of

finding ourselves on the road towards a decrease in the number of our elements, we shall have another, or perhaps two more, to account for and to harmonise.

We now pass to the consideration of researches having no direct connection with the question we have just been handling, but which still indirectly testify in favour of the compound nature of our so-called elements. A law has been proposed which exhibits these "elements" not as a haphazard assemblage of independent bodies, whose number, properties, and atomic weights might have been other than we find them, but as a definite series, or rather group of series, whose members bear to each other relations somewhat similar to the successive grades—*e.g.*, of oxidation—of some one supposed element. The law in question, though it does not remove the teleological difficulties inherent in the respective quantities and the distribution of the simple bodies, solves, at any rate, some of the hitherto unanswered questions which they have put before us. It does more; it enables us to declare not merely that a link is wanting in the series, but to foretell with tolerable accuracy not alone its atomic weight and its specific gravity, but even certain of its reactions. The prevision of phenomena not yet observed has been rightly declared by methodologists to be one of the principal distinctions between a science, in the strict sense of the term, and a mere accumulation of unorganised knowledge. We still hear mention, from time to time, of the splendid triumph achieved by Astronomy, when Leverrier —having from certain observed facts deduced the existence of a planet as yet unknown—was able to calculate its distance, its mass, its orbit and probable position, and when his announcements were found verified on the telescopic examination of the part of the heavens indicated. Such a fulfilment of his forecasts was a verification of astronomical science perfectly intelligible to the outside public. Prof. Mendeleeff, by the application of his "periodic law," was able to foretell distinctly, in 1869, the properties of a metal then unknown, to which he gave the name of "eka aluminium." On the evening of August 27th, 1875, M. Lecoq de Boisbaudran, being engaged with the examination—chemical and spectroscopic—of a blende from the mine of Pierrefitte, discovered a new metal, to which he has given the name of "gallium," in honour of his country. He does not appear to have been acquainted with the predictions of M. Mendeleeff, which, till their startling verification, had not by any means attracted the attention which they undoubtedly merit. The more thoroughly, however, M. Lecoq

de Boisbaudran succeeded in purifying the body which he had obtained, and in accurately determining its properties and reactions, the more closely did it approximate to M. Mendeleeff's "eka aluminium." It must be distinctly understood that the prediction had not been couched in loose generalities like the prophecies of the late Francis Moore, Physician. In 1869 M. Mendeleeff wrote as follows :—"Its atomic weight will be El=68; its oxide, El_2O_3; its salts will present the formula ElX_3. Thus its (only ?) chloride will be $ElCl_3$, yielding on analysis 39 per cent of metal and 61 of chlorine, and will be more volatile than $ZnCl_2$. Its sulphide, El_2S_3, or oxysulphide, $El_2(S,O)_3$, will be precipitable by sulphuretted hydrogen and insoluble in ammonium sulphide.

"The metal will be easily obtained by reduction; its specific gravity will be 5·9, consequently its atomic volume will be 11·5; it will be almost fixed and fusible at a low temperature. It will not become oxidised in contact with the atmosphere, and at a red-heat it will decompose water. The pure metal melted will be slowly attacked by the acids and alkalies. The oxide, El_2O_3, will have the specific gravity 5·5, or thereabouts; it should be soluble in strong acids, form an amorphous hydrate insoluble in water, but soluble in acids and alkalies. The oxide will form neutral and basic salts, $El_2(OH,X)_6$, but not acid salts; its alum, $ElK(SO_4)_2 12H_2O$, will be more soluble than the corresponding salt of aluminium and less crystallisable. The basic properties of El_2O_3 being more decided than those of Al_2O_3, and less than those of ZnO, it will be precipitable by carbonate of baryta. The volatility, as well as the other properties of the saline compounds of El, being the mean between those of aluminium and those of indium, it is probable that the metal will be discovered by means of spectrum analysis, as was the case with indium and thallium."

This very definite account may be read in "Liebig's Annalen" (Supplement-Band viii., p. 133, 1871), and it has indeed been most strikingly confirmed by the properties of the metal as observed by M. Lecoq de Boisbaudran. We must particularly bear in mind that this accord between prediction and observation is evidently becoming more complete as the new metal is obtained in larger quantities and in a state more closely approximating on purity. The celebrated French chemist, indeed, remarks—"Supposing the forecasts of M. Mendeleeff verified altogether, I should have been led to seek for gallium in the precipitates formed by ammonia, and not, as I have done, in the ammoniacal

solutions. In fact, the properties of the hypothetical metal ought to 'present the mean between those of aluminium and indium,' metals whose oxides are almost completely insoluble in ammonia." Yet in a footnote he very materially qualifies this deliverance. He there states—"Oxide of indium is generally considered almost insoluble in ammonia, a property which is utilised in its separation. As for alumina, its solubility in ammonia, though slight, is sensible. It remains to be seen whether the great delicacy of the spectral reaction of gallium, and the minuteness of the quantities upon which I have operated, may not have caused me to over-rate the relative insolubility of gallic oxide in ammonia." We cannot here help pointing out that the solubility of hydrated alumina in ammonia is sufficiently great to vitiate an analysis, even for technological purposes, unless certain well-known precautions are observed. Hence gallia may still be taken up by ammonia to an extent amply sufficient for spectroscopic purposes, even if considerably less soluble in that medium than is alumina.

M. Lecoq de Boisbaudran further observes that without the particular method followed in the present investigation neither the theories of M. Mendeleeff nor his own would have, for a long time, led to the discovery of gallium. With all due deference we must submit that this point is utterly beside the question. If certain theories enable us to foretell correctly the properties of a metal as yet undiscovered, their value is established, and whether the ultimate discovery of such metal was due to the prediction is a mere secondary consideration.

M. Mendeleeff has also announced the probable existence of another metal, to which he gives the name of "eka-silicium," Es = 72, forming an oxide EsO_2. Its properties ought to be intermediate between those of silicium and tin, and it is to be especially sought for among arseniferous and titaniferous minerals or residues. To the discovery of this metal—if metal it may be called, since even tin is relegated among the non-metallic bodies by chemists whose opinions are entitled to respect—we must look forward with anxious interest, not so much for its own sake as for the light which it must throw upon the theory in question.

We must now proceed to an exposition of the law which, in one instance at least, we have seen so signally verified. M. Mendeleeff sets out with a brief reference to the labours of those chemists who have preceded him in this line of enquiry. Gladstone, Cooke, Dumas, Pettenkofer, have all pointed out that the atomic weights of certain groups of the

elements stand in a simple regular ratio to each other. Thus in the calcium group, taking the old atomic weights, we find a progressive increase of approximately 24, the other properties undergoing a correspondingly progressive change. In the sulphur group—sulphur, selenium, and tellurium—the increase of atomic weight on the present scale is approximately 47, with again a progressive modification of properties. Similar developments, which need not here be particularised, have been demonstrated in the potassium, the chlorine, and the phosphorus groups. It has also been noticed, if we are not mistaken, that—although no definite ratio be discoverable—the atomic weights of all the common elements are low, and those of the rarer high. It appears, further, that only the elements of low atomic weight enter into the composition of organised beings. With the very limited exception of copper (Cu = 63) found in the chocolate nut, in the blood of certain crustaceans, and in the feathers of the touraco, and of zinc (Zn = 65) in the ash of a pansy, iron (Fe = 56) has the highest atomic weight of the organic elements.* Again, the elements with high atomic weights, beginning with vanadium (V = 51) and chromium (Cr = 52), with perhaps the single exceptions of manganese and iron, may be regarded as poisonous whenever they exist in a soluble condition. But although such generalisations have not been wanting, there has been no attempt at bringing all the apparently independent groups into harmonious connection. M. Mendeleeff considers that the regular dependence of the properties on changes of the atomic weight appears most clearly on the consideration of dissimilar elements, by the study of which he was led, in 1869, to the discovery of his "periodic law." This law he expressed in the following words:—"The properties of simple bodies, the constitution of their combinations, as well as the properties of the latter, are periodic functions of the atomic weights of the elements." There is one term here so generally misunderstood and misapplied, not merely by persons of good general education, but even by scientific writers, that the meaning of M. Mendeleeff's law will scarcely be understood at first sight. The word "period" is commonly used to signify any portion of

* It may be said that we are using the term "organic element," or "organogen," in a manner different from its usual acceptation. We are perfectly aware of this difference; but we submit that every element really assimilated by plant or animal, and not merely lodged in its tissues as a foreign and hostile intruder,—*e.g.*, mercury in the bones of a votary of blue pill and calomel—has the right to be considered an "organogen." Lead (Pb = 207) has been found in the metallic state in the intestinal canal of insects but there is no evidence whatever in support o its assimilation.

time which we may wish to particularise; but if we remember its derivation we see that it means a "journey round," and that it is applicable only to such portions, of time or of anything else, as exhibit some series of changes tending first in a certain direction and then returning to, or at least towards, the point of departure. Thus a year is legitimately a "period," because in it certain phenomena—astronomical, meteorological, and organic—go through a circle of changes which necessarily ends where it began; but a stretch of 10, or 20, or 100 years cannot be called a period, unless we can discover in it some phenomenon which has its increase and decrease, or its recurrence in such a term of years. In like manner, if we take a group of animals, of minerals, or of elementary bodies, and find that in them some one attribute increases and then decreases again to its former condition, or suffers any other cyclical variation, we may call such a group a "period," and the change in question "periodic." We trust that none of our readers will feel aggrieved at being thus reminded of what doubtless most of them are aware, since a correct understanding on this point is absolutely necessary for the intelligibility of Mendeleeff's law. At the same time we should be very happy to find some word incapable of being misunderstood.

We now turn to Table I., which exhibits the known elements arranged in the arithmetical order of their atomic weights. In the second and third columns we find all whose weights range from 7 to 36. Here we perceive that the characters of the elements change gradually and regularly with alternating magnitudes of the atomic weights. These changes are periodic, taking place in both columns in the same manner, so that the corresponding members are analogous. If we compare respectively—

Li	Be	B	C	N	O	Fl
Na	Mg	Al	Si	P	S	Cl

we must admit that there is, between the members of each of these pairs, a resemblance, which in the cases of carbon and silicon, fluorine and chlorine, is especially striking. We notice, further, that the atomic weight of each member of the first group differs from that of the corresponding member of the second almost exactly by one and the same number, viz., 16. A correspondence of this latter nature, indeed, can no longer be traced between the corresponding members of the remaining columns. If, however, we add the atomic weight of an element in column 3 to that of its representative in column 5, and divide by 2, we obtain an

approximation to the atomic weight of the corresponding member of column 4.

Returning to the second and third series, we find that the corresponding degrees of each have the same atomicity. Only the last four members of each column can combine with hydrogen, forming respectively RH_4, RH_3, RH_2, RH. Thus we see that the power of uniting with hydrogen, absent in the first members of each series, becomes high in the middle and declines again to the end. The permanence or instability of their compounds, their acidity and power to exchange hydrogen for metals, change according to the same gradation. Thus HCl is a well-marked acid, H_2S a much feebler acid, H_3P can no longer be called an acid, and in H_4Si there is no residue of the acid character. In the corresponding members of the first column, again, HFl is manifestly acid, H_2O neutral, H_3N basic, and H_4C neither acid nor basic.

All the members of the second column combine with oxygen, forming salifiable bodies, as—

$$Na_2O,\ Mg_2O_2(MgO),\ Al_2O_3,\ Si_2O_4(SiO_2),\ P_2O_5,\ S_2O_6(SO_3),\ Cl_2O_7;$$

two atoms of the element thus combining with a number of atoms of oxygen, increasing from 1 to 6. This arrangement corresponds with the decrease of basicity and the increase of acidity. At the beginning of the series is a decided base, soda; at the end a powerful acid, the chloric; and in the middle is alumina, which may be either a feeble base or a feeble acid.

A regular connection may be traced, not merely in the combining numbers, but in other chemical and physical characteristics. At the beginning of the first, second, third, and fourth series we find in each case bodies unmistakably metallic; at the opposite end come non-metallic substances, whilst those which stand on the boundary between these two classes take an intermediate position.

Again, if we consider the same set of elements with reference to specific gravity and atomic volume, we find periodic changes. Thus in specific gravity we have—

$$Na=0{\cdot}97,\ Mg=1{\cdot}75,\ Al=2{\cdot}67,\ Si=2{\cdot}49,\ P=1{\cdot}84.$$
$$S=2{\cdot}06,\ Cl\ (liquid)=1{\cdot}33.$$

Here the numbers, as will be seen, increase up to aluminium, and then decrease again, save that sulphur is anomalous, and has a higher specific gravity than phosphorus, as the

theory would require. With the atomic volumes the periodic changes are in the opposite direction; there is first a decrease and then an increase:—

$$Na=24,\ Mg=14,\ Al=10,\ Si=11,\ P=16,\ S=16,\ Cl=27.$$

In their oxides, also, the gradation of specific gravity and atomic volume is to be traced. The specific gravities are—

$$Na_2O=2{\cdot}8,\ Mg_2O_2=3{\cdot}7,\ Al_2O_3=4{\cdot}0,\ Si_2O_4=2{\cdot}6,\ P_2O_5=2{\cdot}7,\ S_2O_6=1{\cdot}9.$$

Here again there is a rise, succeeded by a fall. The series of atomic volumes is—

$$Na_2O=22,\ Mg_2O_2=22,\ Al_2O_3=25,\ Si_2O_4=45,\ P_2O_5=55,\ S_2O_6=82.$$

Here is progressive increase without a return to the original number.

Speaking of alloys, M. Mendeleeff remarks that phosphorus and sulphur have not quite lost the metallic character of the elements at the beginning of the series, since the phosphides and sulphides have still the aspect of alloys. This feature is quite lost in the chlorides.

Turning to another series we find the atomic weights increasing as follows:—

$$Ag=108,\ Cd=112,\ In=110,\ Sn=118,\ Sb=122,\ Te=125,\ I=126.$$

Here again we have a decided metal, silver, at the head of the group; an equally decided non-metallic body, iodine, at the end; and in the middle an element, tin, whose metallic character—paradoxical as it may sound—has been questioned on totally different grounds, and which, according to M. Mendeleeff's scheme, should bear to silicon the same analogy which antimony does to phosphorus, tellurium to sulphur, or iodine to chlorine. The foretold—but as yet undiscovered—element, eka-silicium, is in this case to bridge over the gap, as in the three parallel instances is done by arsenic, selenium, and bromine.

In the silver group (No. 7) we find the specific gravities regularly decreasing as follows:—

$$Ag=10{\cdot}5,\ Cd=8{\cdot}6,\ In=7{\cdot}4,\ Sn=7{\cdot}2,\ Sb=6{\cdot}7,\ Te=6{\cdot}2,\ I=4{\cdot}9.$$

Further instances of "small series," each consisting of seven members, known or unknown, are the following:—

No. 4 . .	K	Ca	—	Ti	V	Cr	Mn
No. 5 . .	Cu	Zn	—	—	As	Se	Br
No. 6 . .	Rb	Sr	—	*Zr*	Nb	Mo	—

Thus M. Mendeleeff considers that all the functions by which the dependencies of the properties of the elements on the atomic weights are expressed appear as periodic. The properties change in accordance with the increasing atomic weights, and are then repeated in a new period with the same regularity as in the former. If we examine his scheme we must admit that it brings admitted relations into a very prominent light. Such groups as fluorine, chlorine, bromine, and iodine; as sulphur, selenium, and tellurium; as nitrogen, phosphorus, arsenic, and antimony; as calcium, strontium, and barium; or as potassium, rubidium, and cæsium,—though their respective members are placed in different series,—fall into positions which well agree with their respective analogies. Even the trinity of the commercial world—gold, silver, and copper—fall in the same horizontal line across the series; gold being a representative of its companion metals in a higher series. A further study of Table II. will bring to light many more curious instances of such representation, which we submit lend a powerful support to M. Mendeleeff's arrangement.

As a matter of course the mere arranging the elements in one arithmetical series would thrust these analogies totally out of sight. As completely would they be hidden if the series were made to consist of any other number of elements. M. Mendeleeff's classification is shown in two tables taken from the translation of his memoir in Liebig's "Annalen." He admits that not all the known elements can be introduced into the small series. Thus in Table I. we see iron, cobalt, nickel, ruthenium, rhodium, palladium, osmium, iridium, and platinum forming an appended eighth group, concerning which the author remarks that they are all grey, all very sparingly fusible, their fusibility increasing in each line from iron to nickel, from ruthenium to palladium, and again from osmium to palladium. Between the corresponding members of the even and the odd small series (except the two first) there is a distinct difference, whilst the corresponding members of the even and again of the odd, series show a close analogy.

Table I.—Elements in Small Series.

Series.	First Group. — R_2O.	Second Group. — RO.	Third Group. — R_2O_3.	Fourth Group. RH_4 RO_2.	Fifth Group. RH_3 R_2O_5.	Sixth Group. RH_2 RO_3.	Seventh Group. RH R_2O_7.	Eighth Group. (R_2H) (RO_4),			
1	1 H	—	—	—	—	—	—				
2	Li 7	Be 9	B 11	C 12	N 14	O 16	F 19				
3	23 Na	24 Mg	27 Al	28 Si	31 P	32 S	35 Cl				
4	K 39	Ca 40	? 44	Ti 48	V 51	Cr 52	Mn 55	Fe 56	Co 59	Ni 59	Cu 63
5	(63 Cu)	65 Zn	68 ?	72 ?	75 As	78 Se	80 Br				
6	Rb 85	Sr 87	Yt 88	Zr 90	Nb 94	Mo 96	? 100	Ru 104	Rh 104	Pd 106	Ag 108
7	(108 Ag)	112 Cd	113 In	118 Sn	122 Sb	125 Te	127 I				
8	Cs 133	Ba 137	? Di 138	Ce 140	—	—	—	Os 195	Ir 197	Pt 198	Au 199
9	—	—	—	—	—	—	—				
10	—	—	Er 178	? La 180	Ta 182	W 184	? 190	—	—	—	—
11	(199 Au)	200 Hg	204 Tl	207 Pb	208 Bi	—	—				
12	—	—	—	Th 231	—	U 240	—	—	—	—	—

Table II.—Elements in Great Series.

I.	II.	III.	IV.	V.	VI.	VII.	VIII.
			Potassium = 39	Rubidium = 86	Cæsium = 133	—	—
			Calcium = 40	Strontium = 87	Barium = 137	—	—
			—	Yttrium = 88	Didymium = 138	Erbium = 178	—
			Titanium = 48	Zirconium = 90	Cerium = 140	Lanthanum = 180	Thorium = 231
			Vanadium = 51	Niobium = 94	—	Tantalum = 182	—
			Chromium = 52	Molybdenum = 96	—	Tungsten = 184	Uranium = 240
			Manganese = 55	—	—	—	
			Iron = 56	Ruthenium = 104	—	Osmium = 195	—
			Cobalt = 59	Rhodium = 104	—	Iridium = 197	—
			Nickel = 59	Palladium = 106	—	Platinum = 198	—
			Copper = 63	Silver = 108	—	Gold = 199	—
			Zinc = 65	Cadmium = 112	—	Mercury = 200	—
			—	Indium = 113	—	Thallium = 204	—
			—	Tin = 118	—	Lead = 207	—
			Arsenic = 75	Antimony = 122	—	Bismuth = 208	—
			Selenium = 78	Tellurium = 125	—	—	—
			Bromine = 80	Iodine = 127	—	—	—

Typical.

Hydrogen = 1

Lithium	= 7	Sodium = 23
Beryllium	= 9·4	Magnesium = 24
Boron	= 11	Aluminium = 27
Carbon	= 12	Silicon = 28
Nitrogen	= 14	Phosphorus = 31
Oxygen	= 16	Sulphur = 32
Fluorine	= 19	Chlorine = 35

Thus if we recur to the four series above mentioned :—

No. 4 . .	K	Ca	—	Ti	V	Cr	Mn
No. 5 . .	Cu	Zn	—	—	As	Se	Br
No. 6 . .	Rb	Sr	—	Zr	Nb	Mo	—
No. 7 . .	Ag	Cd	In	Sn	Sb	Te	I

Here it is at once evident that there a decided analogy prevails between the corresponding members of Nos. 4 and 6, and again of Nos. 5 and 7. But between Nos. 4 and 5, 5 and 6, or 6 and 7, there is no such analogy.

Table II. shows the elements as arranged in "great series." In Series I. stands hydrogen alone. This reminds us of a remark made lately by Prof. F. Guthrie, that very generally the commonest bodies are the most anomalous. Series II. contains the seven elements from lithium to fluorine inclusive, these two first groups being designated "typical." Series III. contains also seven elements. Series IV. contains fourteen bodies, which, when the three blanks indicated by M. Mendeleeff have been filled up, as one of them already is by the discovery of gallium (eka-aluminium), will be raised to seventeen.

This "great series" consists of the two "small series," No. 4 and No. 5, with three of the non-serial elements—iron, cobalt, and nickel—interposed between them. The great series No. 5 is composed of sixteen known elements, and a blank space is left for a body not yet discovered. The small series here embraced are Nos. 6 and 7, and again three of the non-serial elements—ruthenium, rhodium, and palladium—are interpolated. The great series VI. is very imperfect, comprehending only four elements, which are placed parallel with the first four bodies in Nos. IV. and V. Great series VII. is more perfect; it contains parts of two small series, and again three non-serial elements—osmium, indium, and platinum. Here, however, we are struck with a difficulty: between the atomic weights of tungsten (W=184) and osmium (Os=195) there is the gap of 11. Now if M. Mendeleeff concluded that two undiscovered elements, eka-aluminium and eka-silicium, must exist to fill up the interval of 10 between Zn=65 and As=75, in great series IV., why may we not with equal right calculate on the discovery here also of *two* unknown bodies? But should this be hereafter found to be the fact, the correspondence between the members of Series V. and VII. will be deranged. Gold will occupy the position corresponding not with silver, but with cadmium; mercury

will figure as the representative of indium; thallium, of tin; lead, of antimony; and bismuth, of tellurium; changes which will not commend themselves to the approval of chemists. Altogether we find that the difference between the successive atomic weights, in parts of the series where M. Mendeleeff does not suspect—or at least does not indicate—an absent element, ranges from 1 to 5. What will then become of the interesting harmonies which the scheme exhibits if some undiscovered element should crop up to fill some of the larger of these intervals? We certainly do not find that the author shows any reason why such an event should be improbable. M. Mendeleeff himself declares that the Periodic Law cannot be harmonised with the Atomic theory without inverting known facts.

It will, we think, be evident to the reader that—splendidly as the discovery of gallium has fulfilled the deductions drawn from the law under consideration—the subject is still in its infancy, and that future research is abundantly needed to confirm, to modify, or to extend. We should suggest to chemists and physicists a course parallel to what we have recommended to biologists in the case of the Darwinian hypothesis. We would say—Take up the law provisionally, and work with it. One of the most essential steps is the search for "eka-silicium," conducted, as the author proposes, by the spectroscopic examination of titaniferous minerals and residues. The contents of certain sealed papers which M. Lecoq de Boisbaudran has deposited with the Institute, and in which he has explained his special ideas on the classification of the elementary bodies, must be awaited with interest. Readers who would wish to pursue this subject further will find detailed information in the "Journal de la Soc. Chimique Russe" (i., p. 60); in Liebig's "Annalen" (Supplement-Band viii., p. 183, 1871); and, if they understand the Russian language, in M. Mendeleeff's work, "Foundations of Chemistry" (vol. ii.). As far as the "periodic law" may be considered established, it seems to us most decidedly to contradict the hypothesis of some sixty primordially distinct bodies, and to exhibit our present elements as products of the evolution of something to us yet unknown. For that something it must be the task of chemists to search.

II. ON THE PROBABLE ORIGIN AND AGE OF THE SUN.

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THE total annual amount of radiation from the whole surface of the sun is 8340×10^{30} foot-pounds. To maintain the present rate of radiation it would require the combustion of about 1500 lbs. of coal per hour on every square foot of the sun's surface; and were the sun composed of that material it would all be consumed in less than 5000 years. The opinion that the sun's heat is maintained by combustion cannot be entertained for a single moment. Mr. Lockyer has suggested that the elements of the sun are, owing to its excessive temperature, in a state of dissociation, and some have supposed that this fact might help to explain the duration of the sun's heat. But it must be obvious that, even supposing we were to make the most extravagant estimate of the chemical affinities of these elements, the amount of heat derived from their combination could at most give us only a few thousand years additional heat. Under every conceivable supposition the combustion theory must be abandoned.

It is now generally held by physicists that the enormous store of heat possessed by the sun could only have been derived from gravitation. For example, a pound of coal falling into the sun from an infinite distance would produce by its concussion more than 6000 times the amount of heat that would be generated by its combustion. It would, in fact, amount to upwards of 65,000,000,000 foot-pounds—an amount of energy sufficient to raise 1000 tons to a height of $5\frac{1}{2}$ miles.

There are two forms in which the gravitation theory has been presented: the first, the meteoric theory, propounded by Dr. Meyer; and the second, the contraction theory, advocated by Helmholtz. The meteoric theory of the sun's heat has now been pretty generally abandoned for the contraction theory advanced by Helmholtz. Suppose, with Helmholtz, that the sun originally existed as a nebulous mass, filling the entire space presently occupied by the solar system, and extending into space indefinitely beyond the

outermost planet. The total amount of work in foot-pounds performed by gravitation in the condensation of this mass to an orb of the sun's present size can be found by means of the following formula given by Helmholtz :—

$$\text{Work of condensation} = \frac{3}{5} \cdot \frac{r^2 M^2}{Rm} \cdot g.$$

M is the mass of the sun, m the mass of the earth, R the sun's radius, and r the earth's radius. Taking—

$M = 4230 \times 10^{27}$ lbs., $m = 11{,}920 \times 10^{21}$ lbs.,
$R = 2{,}328{,}500{,}000$ feet, and $r = 20{,}889{,}272$ feet,

we have then, for the total amount of work performed by gravitation in foot-pounds,

$$\frac{3}{5} \cdot \frac{(20{,}889{,}272{\cdot}5)^2 \times (4230 \times 10^{27})^2}{2{,}328{,}500{,}000 \times 11{,}920 \times 10^{21}}$$
$$= 168{,}790 \times 10^{36} \text{ foot-pounds.}$$

The amount of heat thus produced by gravitation would suffice for 20,237,500 years.

The conclusions are based upon the assumption that the density of the sun is uniform throughout. But it is highly probable that the sun's density increases towards the centre, in which case the amount of work performed by gravitation would be something more than the above.

At this point, in reference to the age of our globe, Geology and Physics are generally supposed to come into direct antagonism. For if it be true, as physicists maintain, that gravitation is the only possible source from which the sun could have derived its store of energy, then the sun could not have maintained our globe at its present temperature for more than about 20 millions of years. "On the very highest computation which can be permitted," says Prof. Tait, "it cannot have supplied the earth, even at the present rate, for more than about fifteen or twenty million years."* The limit to the age of the sun's heat must have limited the age of the habitable globe. All the geological history of the globe would necessarily be comprehended within this period. If the sun derived its heat from the condensation of its mass, then it could not possibly be more than about twenty million years since the beginning of the Laurentian period. But twenty million years would be considered by most geologists to represent only a comparatively small portion of the time which must have elapsed since organic life began on our globe.

* Recent Advances in Physical Science, p. 175.

It is true that the views which formerly prevailed amongst geologists, in regard to the almost unlimited extent of geological time, have of late undergone very considerable modifications; but there are few geologists, I presume, who would be willing to admit that the above period is sufficient to comprehend the entire history of stratified rocks.

It is the facts of denudation which most forcibly impress the mind with a sense of immense duration, and show most convincingly the great antiquity of the earth.

We know unquestionably that many of the greatest changes undergone by the earth's crust were produced, not by convulsions and cataclysms of nature, but by those ordinary agencies that we see at work every day around us, such as rain, snow, frost, ice, chemical action, &c. Valleys have not been produced by violent dislocations, nor the hills by upheavals, but both have been carved out of the solid rock by the silent and gentle agency of chemical action, frost, rain, ice, and running water. In short, the rocky face of our globe has been moulded into hill and dale, and ultimately worn down to the sea-level by means of these apparently trifling agents, not merely once or twice, but probably dozens of times over during past ages. Now when we reflect that with such extreme slowness do these agents perform their work that we might, if we could, watch their operations from year to year, and from century to century, without being able to perceive that they make any sensible impression, we are necessitated to conclude that geological periods must be enormous. The utter inadequacy of a period of 20 million years for the age of our earth is demonstrable from the enormous thickness of rock which is known to have been removed off certain areas by denudation. I shall now briefly refer to a few of the many facts which might be adduced on this point.

One plain and obvious method of showing the great extent to which the general surface of the country has been lowered by denudation is furnished, as is well known, by the way in which the inequalities of surface produced by faults or dislocations have been effaced. It is quite common to meet with faults where the strata on the one side have been depressed several hundreds—and in some cases thousands—of feet below that on the other, but we seldom find any indications of such on the surface, the inequalities on the surface having been all removed by denudation. But in order to effect this a mass of rock must have been removed equal in thickness to the extent of the dislocation. The following are a few examples of large faults:—

The great Irwell fault, described by Prof. Hull,* which stretches from the Mersey west of Stockport to the north of Bolton, has a throw of upwards of 3000 feet.

Some remarkable faults have been found by Prof. Ramsay in North Wales. For example, near Snowdon, and about a mile E.S.E. of Beddgelert, there is a fault with a downthrow of 5000 feet ; and in the Berwyn Hills, between Bryn-mawr and Post-gwyn, there is one of 5000 feet. In the Aran Range there is a great fault, designated the Bala fault, with a downthrow of 7000 feet. Again, between Aran Mowddwy and Careg Aderyn the displacement of the strata amounts to no less than from 10,000 to 11,000 feet.† Here we have evidence that a mass of rock, varying from 1 mile to 2 miles in vertical thickness, must have been denuded in many places from the surface of the country in North Wales.

The fault which passes along the east side of the Pentlands is estimated to have a throw of upwards of 3000 feet.‡ Along the flank of the Grampians a great fault runs from the North Sea at Stonehaven to the estuary of the Clyde, throwing the Old Red Sandstone on end sometimes for a distance of 2 miles from the line of dislocation. The amount of the displacement, Prof. Geikie‖ concludes, must be in some places not less than 5000 feet, as indicated by the position of occasional outlyers of conglomerate on the Highland side of the fault.

The great fault crossing Scotland from near Dunbar to the Ayrshire coast, and which separates the Silurians of the South of Scotland from the Old Red Sandstone and Carboniferous tracts of the North, has been found, by Mr. B. N. Peach, of the Geological Survey,§ to have in some places a throw of fully 15,000 feet. This great dislocation is older than the Carboniferous period, as is shown by the entire absence of any Old Red Sandstone on the south side of the fault, and by the occurrence of the Carboniferous Limestone and Coal-measures lying directly on the Silurian rocks. We obtain here some idea of the enormous amount of denudation which must have taken place during a comparatively limited geological epoch. So vast a thickness of Old Red Sandstone could not, as Mr. Peach remarks, "have ended originally where the fault now is, but must have swept southwards over the Lower Silurian uplands. Yet these thousands of feet of sandstones, conglomerates, lavas, and

* Mem. Geol. Survey of Lancashire, 1862.
† Mem. Geol. Survey of Great Britain, vol. iii.
‡ Memoir to Sheet 32, Geol. Survey Map of Scotland.
‖ Nature, vol. xiii., p. 390.
§ Explanation to Sheet 15, Geol. Survey Map of Scotland.

tuffs were so completely removed from the south side of the fault previous to the deposition of the Carboniferous Limestone series and the Coal-measures that not a fragment of them is anywhere to be seen between these latter formations and the old Silurian floor." This enormous thickness of nearly 3 miles of Old Red Sandstone must have been denuded away during the period which intervened between the deposition of the Lower Old Red Sandstone and the accumulation of the Carboniferous Limestone.

Near Tipperary, in the south of Ireland, there is a dislocation of the strata of not less than 4000 feet,* which brings down the Coal-measures against the Silurian rocks. Here 1000 feet of Old Red Sandstone, 3000 feet of Carboniferous Limestone, and 800 feet of Coal-measures have been removed by denudation off the Silurian rocks. Not only has this immense thickness of beds been carried away, but the Silurian itself on which they rested has been eaten down in some places into deep valleys several hundreds of feet below the surface on which the Old Red Sandstone rested.

Faults to a similar extent abound on the Continent and in America, but they have not been so minutely examined as in this country. In the Valley of Thessolon, to the north of Lake Huron, there is a dislocation of the strata to the extent of 9000 feet.†

In front of the Chilowee Mountains there is a vertical displacement of the strata of more than 10,000 feet.‡ Prof. H. D. Rogers found in the Appalachian coal-fields faults ranging from 5000 feet to more than 10,000 feet of displacement.

There are other modes than the foregoing by means of which geologists are enabled to measure the thickness of strata which may have been removed in places off the present surface of the country, into the details of which I need not here enter. But I may give a few examples of the enormous extent to which the country, in some places, has been found to have been lowered by denudation.

Prof. Geikie has shown‖ that the Pentlands must at one time have been covered with upwards of a mile in thickness of Carboniferous rocks which have all been removed by denudation.

In the Bristol coal-fields, between the River Avon and the Mendips, Prof. Ramsay has shown§ that about 9000 feet of

* Jukes's and Geikie's Manual of Geology, p. 441.
† Geology of Canada, 1863, p. 61.
‡ Safford's Geology of Tennessee, p. 309.
‖ Mem. to Sheet 32, Geol. Survey of Scotland.
§ "Denudation of South Wales." Memoirs of Geol. Survey, vol. i.

Carboniferous strata have been removed by denudation from the present surface.

Between Bendrick Rock and Garth Hill, South Glamorganshire, a mass of Carboniferous and Old Red Sandstone, of upwards of 9000 feet, has been removed. At the Vale of Towy, Caermarthenshire, about 6000 feet of Silurian and 5000 feet of Old Red Sandstone—in all about 11,000 vertical feet—have been swept away. Between Llandovery and Aberaeron a mass of about 12,000 vertical feet of the Silurian series has been removed by denudation. Between Ebwy and the Forest of Dean, a distance of upwards of 20 miles, a thickness of rock varying from 5000 to 10,000 feet has been abstracted.

Prof. Hull found* on the northern flanks of the Pendle Range, Lancashire, the Permian beds resting on the denuded edges of the Millstone Grit, and these were again observed resting on the Upper Coal-measures south of the Wigan coal-field. Now, from the known thickness of the Carboniferous series in this part of Lancashire, he was enabled to calculate approximately the quantity of Carboniferous strata which must have been carried away between the period of the Millstone Grit and the deposition of the Permian beds, and found that it actually amounted to no less than 9,900 feet. He also found in the Vale of Clitheroe, and at the base of the Pendle Range, that the Coal-measures, the whole of the Millstone grit, the Yoredale series, and part of the Carboniferous Limestone, amounting in all to nearly 20,000 feet, had been swept away—an amount of denudation which, as Prof. Hull remarks, cannot fail to impress us with some idea of the prodigious lapse of time necessary for its accomplishment.

In the Nova Scotia coal-fields one or two miles in thickness of strata have been removed in some places.†

It may be observed that, enormous as is the amount of denudation indicated by the foregoing figures, these figures do not represent in most cases the actual thickness of rock removed from the surface. We are necessitated to conclude that a mass of rock equal to the thickness stated must have been removed, but we are in most cases left in uncertainty as to the total thickness which has actually been carried away. In the case of a fault, for example, with a displacement of (say) one mile, where no indication of it is seen at the surface of the ground, we know that on one side of the fault a thickness of rock equal to one

* Quart. Journ. Geol. Soc., vol. xxiv., p. 323.
† Lyell's Student's Manual, chap. 23.

mile must have been denuded, but we do not know how much more than that may have been removed. For anything which we know to the contrary hundreds of feet of rock may have been removed before the dislocation took place, and as many more hundreds after all indications of dislocation had been effaced at the surface.

But it must be observed that the total quantity of rock which has been removed from the *present* surface of the land is evidently small in proportion to the total quantity removed during the past history of our globe. For those thousands and thousands of feet of rock which have been denuded were formed out of the waste of previously existing rocks, just as these had been formed out of the waste of yet older rock-masses. In short, as a general rule, the rocks of one epoch have been formed out of those of preceding periods, and go themselves to form those of subsequent epochs.

In many of the cases of enormous denudation to which we have referred, the erosion has been effected during a limited geological epoch. We have, for example, seen that upwards of a mile in thickness of Carboniferous rock has been denuded in the area of the Pentlands. But the Pentlands themselves, it can be proved, existed as hills, in much their present form, before the Carboniferous rocks were laid down over them; and as they are of Lower Old Red Sandstone age, and have been formed by denudation, they must consequently have been carved out of the solid rock between the period of the Old Red Sandstone and the beginning of the Carboniferous age. This affords us some conception of the immense lapse of time represented by the Middle and Upper Old Red Sandstone periods.

Again, in the case of the great fault separating the Silurians of the south of Scotland from the Old Red Sandstone tracts lying to the north, a thickness of the latter strata of probably more than a mile, as we have seen, must have been removed from the ground to the south of the fault before the commencement of the Carboniferous period. And again, in the case of the Lancashire coal-fields, to which reference has been made, nearly two miles in thickness of strata had been removed in the interval which elapsed between the Millstone Grit and the Permian periods.

As we are enabled, from geological evidence, to form some rough estimate of the extent to which the country in various places has been lowered by sub-aërial denudation during a given epoch, it is evident that we should have a means of arriving at some idea of the length of that epoch, did we

know the probable rate at which the denudation took place. If we had a means of forming even the roughest estimate of the probable average rate of sub-aërial denudation during past ages, we should be enabled thereby to assign approximately an inferior limit to the age of the stratified rocks. We could then tell, at least, whether the amount of sub-aërial denudation known to have been effected during past geological ages could have been accomplished within 20 million years or not, and this is about all with which we are at present concerned. And if it can be proved that a period of 20 millions of years is much too short to account for the amount of denudation known to have taken place, then it is certain that the gravitation theory cannot explain the origin and source of the sun's heat.

A very simple and obvious method of determining the present mean rate of sub-aërial denudation was pointed out several years ago,* viz., that the rate of denudation must be equal to the rate at which the materials are carried off the land into the sea. But the rate at which the materials are thus abstracted is measured by the rate at which sediment is carried down by our rivers. Consequently, in order to determine the present rate of sub-aërial denudation, we have only to ascertain the quantity of sediment annually carried down by the river systems.

Very accurate measurements have been made of the quantity of sediment carried down into the Gulf of Mexico by the River Mississippi, and it is found to amount to 7,474,000,000 cubic feet. The area drained by the river is 1,224,000 square miles. Now 7,474,000,000 cubic feet removed from 1,224,000 square miles of surface is equal to 1-4566th of a foot off the surface per annum, or 1 foot in 4566 years. The specific gravity of the sediment is taken at 1·9, and that of the rock at 2·5; consequently the amount removed is equal to 1 foot of rock in about 6000 years. For many reasons there are few rivers better adapted for affording us a fair average of the rate of sub-aërial denudation than the Mississippi. In reference to the above I may here quote the words of Sir Charles Lyell:—"There seems," he says, "no danger of our over-rating the mean rate of waste by selecting the Mississippi as our example, for that river drains a country equal to more than half the continent of Europe, extends through 20 degrees of latitude, and therefore through regions enjoying a great variety of climate, and some of its tributaries descend from mountains of great height. The

* Phil. Mag., May, 1868; Feb., 1867. Climate and Time, chap. 20. See also Trans. Geol. Soc. of Glasgow, vol. iii.

Mississippi is also more likely to afford us a fair test of ordinary denudation, because, unlike the St. Lawrence and its tributaries, there are no great lakes in which the fluviatile sediment is thrown down and arrested on its way to the sea." *

Rough estimates have been made of the sediment carried down by some eight or ten European rivers; and although those estimates cannot be depended upon as being anything like accurate, still they show that it is extremely probable that the European continent is being denuded at about the same rate as the American.

I think we may safely assume, without the risk of any great error, that the average rate of sub-aërial denudation during past geological ages did not differ much from the present. The rate at which a country is lowered by sub-aërial denudation is determined† not so much by the character of its rocks as by the sedimentary carrying power of its river systems. And this again depends mainly upon the amount of rain-fall, the slope of the ground, and the character of the soil and vegetation covering the surface of the country. And in respect of these we have no reason to believe that the present is materially different from the past. No doubt the average rain-fall during some past epochs might have been greater than at present, but there is just as little reason to doubt that during other epochs it might have been less than now. We may therefore conclude that about one foot of rock removed from the general surface of the country in 6000 years may be regarded as not very far from the average rate of denudation during past ages.

But some of the cases we have given of great denudation refer to comparatively small areas, and others to beds which form anticlinal axes, and which, as is well known, denude more rapidly than either synclinal or horizontal beds. We shall therefore—to prevent the possibility of over-estimating the length of time necessary to effect the required amount of denudation—assume the rate to have been double the above, or equal to one foot in 3000 years.

To lower the country one mile by denudation would therefore require, according to the above rate, about 15 million years; but we have seen that a thickness of rock more than equal to that must have been swept away since the Carboniferous period. For even during the Carboniferous period itself more than a mile in thickness of strata in many places was removed. Again, there can be no doubt whatever that

* Student's Manual of Geology, p. 91 (second edition).

† *See* Climate and Time, p. 334.

the amount of rock removed during the Old Red Sandstone period was much greater than one mile; for we know perfectly well that over large tracts of country nearly a mile in thickness of rock was carried away between the period of the Lower Old Red Sandstone and the Carboniferous epoch. Further, all geological facts go to show that the time represented by the Lower Old Red Sandstone itself must have been enormous.

Now, three miles of rock removed since the commencement of the Old Red Sandstone period (which in all probability s an under-estimate) would give us 45 million years.

Again, going further back, we find the lapse of time represented by the Silurian period to be even more striking than that of the Old Red Sandstone. The uncomformities in the Silurian series indicate that many thousands of feet of these strata were denuded before overlying members of the same great formations were deposited. And again, this immense formation was formed in the ocean by the slow denudation of pre-existing Cambrian continents, just as these had been built up out of the ruins of the still prior Laurentian land. And even here we do not reach the end of the series, for the very Laurentians themselves resulted from the denudation not of the primary rocks of the globe, but of previously existing sedimentary and probably igneous rocks of which, perhaps, no recognisable portion now remains.

Few familiar with the facts of geology will consider it too much to assume that the time which had elapsed prior to the Old Red Sandstone was equal to the time which has elapsed since that period. But if we make this assumption, this will give us at least 90 million years as the age of the stratified rocks.

That the foregoing is not an over-estimate of the probable amount of rock removed by sub-aërial denudation during past geological ages will appear further evident from the following considerations:—The mountain ridges of our globe, in most cases, as is well known, have been formed by sub-aërial denudation: they have been carved out of the solid block. They stand two thousand, four thousand, or five thousand feet high, as the case may be, simply because two thousand, four thousand, or five thousand feet of rock have been denuded from the surrounding country. The mountains are high simply because the country has been lowered. But it must be observed that the height which the mountains reach above the surrounding country does not measure the full extent to which the country has been lowered by denudation, because the mountains themselves have also been lowered.

The height of the mountains represents merely the extent to which the country has been lowered. In the formation of a mountain by denudation, say 3000 feet in height, probably more than 6000 feet of strata may have been removed from the surrounding country. The very fact of a mountain standing above the surrounding country exposes it the more to denudation, and it is certainly not an exaggerated assumption to suppose that whilst the general surface of the country was being lowered 6000 feet by denudation, the mountain itself was at least lowered by 3000 feet.

The very common existence of mountains two or three thousand feet in height, formed by sub-aërial denudation, proves that at least one mile must have been worn off the general surface of the country. It does not, of course, follow that the general surface ever stood at an elevation of one mile above the sea-level, since denudation would take place as the land gradually rose. We know that the land was once under the sea, for it was there that it was formed. It is built up out of the materials resulting from the carving out into hill and dale, through countless ages, of a previously existing land, just as this latter had resulted from the destruction of a still older land, and so on in like manner back into the unknown past.

It has now been proved, by the foregoing very simple and obvious method, that the age of the earth must be far more than 20 or 30 million years. This method, it is true, does not enable us to determine with anything like accuracy the actual age of the globe, but it enables us to determine with absolute certainty that it must be far greater than 20 million years. We have not sufficient data to determine how many years have elapsed since life began on the globe, for we do not know the total amount of rock removed by denudation; but we have data perfectly sufficient to show that it began far more than twice 20 million years ago.

But if the present order of things has been existing for more than 20 million years, then the sun must have been illuminating our globe for that period, and, if so, then there must have been some other source than that of gravitation from which the sun derived its energy, for gravitation, as we have seen, could only have supplied the present rate of radiation for about one-half that period.

It is perfectly true, as has been stated, that the length of time that the sun could, by its radiation, have kept the earth in a state fit for animal and vegetable life, must have been limited by the store of energy in the form of hea which it possessed. But it does not follow as a necessary conse-

quence, as is generally supposed, that this store of energy must have been limited to the amount obtained from gravity in the condensation of the sun's mass. The utmost that any physicist is warranted in affirming is simply that it is impossible for him to *conceive* of any other source. His *inability*, however, to conceive of another source cannot be accepted as a proof that there *is* no other source. But the physical argument that the age of our earth must be limited by the amount of heat which could have been received from gravity is in reality based upon this assumption—that, because no other source can be conceived of, there is no other source.

It is perfectly obvious, then, that this mere negative evidence against the possibility of the age of our habitable globe being more than 20 or 30 million years is of no weight whatever when pitted against the positive evidence here advanced, that its age must be far greater.

Now, in proving that the antiquity of our habitable globe must be far greater than 20 or 30 million years, we prove that there must have been some other source in addition to gravity from which the sun derived his store of energy; *and this is the point which I have been endeavouring to reach by this somewhat lengthy discussion.*

Are we really under any necessity of assuming that the sun's heat was wholly, or even mainly, derived from the condensation of his mass by gravity? According to Helmholtz's theory of the origin of the sun's heat by condensation, it is assumed that the matter composing the sun, when it existed in space as a nebulous mass, was not originally possessed of temperature, but that the temperature was given to it as the mass became condensed under the force of gravitation. It is supposed that the heat given out was simply the heat of condensation. But it is quite conceivable that the nebulous mass might have been possessed of an original store of heat previous to condensation.

It is quite possible that the very reason why it existed in such a rarefied or gaseous condition was its excessive temperature, and that condensation only began to take place when the mass began to cool down. It seems far more probable that this should have been the case than that the mass existed in so rarefied a condition without temperature. For why should the particles have existed in this separate form when devoid of the repulsive energy of heat, seeing that, in virtue of gravitation, they had such a tendency to approach one another?

It will not do to begin with the assumption of a cold nebulous mass, for, the moment that the mass existed as

such, condensation—under the influence of the mutual attraction of its particles—would commence. We must therefore assume either that the mass was created at the moment condensation began, or that, prior to this moment, it existed under some other form. There are few, I think, who would be willing to adopt the former alternative. If we adopt the latter we must then ask the question, In what condition did this mass exist prior to the commencement of condensation? The answer to this question would naturally be that it existed in a condition of excessive temperature, the repulsive force of heat preventing the particles approaching one another. In short, the excessive temperature was the very cause of the nebulous condition.

But if the mass was originally in a heated condition, then in condensing it would have to part not only with the heat of condensation, but also with the heat which it originally possessed.

It is therefore evident that if we admit that the nebulous mass was in a state of incandescence prior to condensation, it will really be difficult to fix any limit either to the age of the sun or to the amount of heat which it may have originally possessed. The 20 million years' heat obtained by condensation may in such a case be but a small fraction of the total quantity possessed by the mass.

The question now arises—By what means could the nebulous mass have become incandescent? From what source could the heat have been obtained? The dynamical theory of heat affords, as was shown several years ago,* an easy answer to this question. The answer is that *the energy in the form of heat possessed by the mass may have been derived from Motion in Space*. Two bodies, each one-half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, would by their concussion generate in a *single moment* 50 million years' heat. For two bodies of that mass, moving with a velocity of 476 miles per second, would possess 4149×10^{38} foot-pounds of kinetic energy, and this converted into heat by the stoppage of their motion would give out an amount of heat which would cover the present rate of the sun's radiation for a period of 50 million years.

Why may not the sun have been composed of two such bodies? And why may not the original store of heat possessed by him have all been derived from the concussion of these two bodies? Two such bodies coming into collision with that velocity would be dissipated into vapour and converted into a nebulous mass by such an inconceivable amount of heat as would thus be generated; and when

* Phil. Mag. for May, 1868.

condensation on cooling took place, a spherical mass like that of the sun would result. It is perfectly true that two such bodies could never attain the required amount of velocity by their mutual gravitation towards each other. But there is no necessity whatever for supposing that their velocities were derived from their mutual attraction alone: they might have been approaching each other with the required velocity wholly independent of gravitation.

We know nothing whatever regarding the absolute motion of bodies in Space; and, beyond the limited sphere of our observation, we know nothing even of their relative motions. There may be bodies moving in relation to our system with inconceivable velocity. For anything that we know to the contrary, were one of these bodies to strike our earth the shock might be sufficient to generate an amount of heat that would dissipate the earth into vapour, though the striking body might not be heavier than a cannon-ball. There is, however, nothing very extraordinary in the velocity which we have found would be required to generate the 50 million years' heat in the case of the two supposed bodies. A comet having an orbit extending to the path of the planet Neptune, approaching so near the sun as to almost graze his surface in passing, would have a velocity of about 390 miles per second, which is within 86 miles of that required.

It must be borne in mind, however, that the 476 miles per second is the velocity at the moment of collision; but more than one-half of this would be derived from the mutual attraction of the two bodies in their approach to each other. Suppose, for simplicity of calculation, each body to be equal in volume to the sun, and of course one-half the density, the amount of velocity which they would acquire by their mutual attraction would be 274 miles per second. Consequently we have to assume an original or projected velocity of only 202 miles per second. And if the original velocity was 676 per second, the total amount of heat generated would suffice for 200 million years at the present rate of radiation.

On former occasions* I expressed it as my opinion that the total quantity of heat possessed by the sun could not probably exceed 100 million years' heat. But if we admit that the heat was derived from Motion in Space, there really does not seem any reason why it may not be double that amount.

It will be asked—Where did the two bodies get their motion? It may as well, however, be asked—Where did

* Phil. Mag., May, 1868. Climate and Time, chap. 21.

they get their existence? It is just as easy to conceive that they always existed in motion as to conceive that they always existed at rest. In fact, this is the only way in which energy can remain in a body without dissipation into Space. Under other forms a certain amount of the energy is constantly being transformed into heat which never can be re-transformed back again, but is dissipated into Space as radiant heat. But a body moving in void stellar space will, unless a collision takes place, retain its energy in the form of motion untransformed for ever.

It will perhaps be urged as an objection that we have no experience of bodies moving in space with velocities approaching to anything like 400 or 600 miles per second. A little consideration will, however, show that this is an objection which can hardly be admitted, as we are not in a position to be able to perceive bodies moving with such velocities. No body moving at the rate of 400 miles per second could remain as a member of our solar system. Beyond our system, the only bodies visible to us are the nebulæ and fixed stars, and they are visible because they are luminous. But the fixed stars are beyond doubt suns similar to our own; and if we assume that the energy in the form of heat and light possessed by our sun has been derived from Motion in Space, we are hardly warranted in denying that the light and heat possessed by the stars were derived from another source. It is true that the motion of the stars in relation to one another, or in relation to our system (and this is the only motion known to us), is but trifling in comparison to what we even witness in our solar system. But this is what we ought, *à priori*, to expect; for if their light and heat were derived from Motion in Space, like that of our sun, then, like the sun, they must have lost their motion. In fact, *they are suns, and visible because they have lost their motion.* Had not the masses of which these suns were composed lost their motion they would have been non-luminous, and of course totally invisible to us. In short, we only see in stellar space those bodies which, by coming into collision, have lost their motion, for it is the lost motion which renders them luminous and visible.*

* When the foregoing theory of the origin of the sun's heat was advanced, in 1868, I was not aware that a paper on the "Physical Constitution of the Sun and Stars" had been read before the Royal Society by Mr. G. Johnstone Stoney, in which he suggested that the heat possessed by the stars may have been derived from collisions with one another. "If two stars," he says, should be brought by their proper motion very close, one of three things would happen:—Either they would pass quite clear of one another, in which case they would recede to the same immense distance asunder from which they had come; or they would become so entangled with one another as to emerge

The formation of a sun by collision is an event that would not be likely to escape observation if it occurred within the limits of visibility in space. But such an event must be of very rare occurrence, or the number of stars visible would be far greater than it is. The number of stars registered down to the seventh magnitude, inclusive, is—according to Herschel—somewhere between 12,000 and 15,000, and this is all that can possibly be seen by the naked eye. Now, if we suppose each of them to shine like our sun for (say) 100 million years, then one formed in every 7000 or 8000 years would maintain the present number undiminished. But this is the number included in both hemispheres, so that the occurrence of an event of such unparalleled splendour and magnificence as the formation of a star or rather nebula—for this would be the form first assumed—is what can only be expected to be seen on our hemisphere once in about 15,000 years.

The absence of any historical record of such an event having ever occurred can therefore be no evidence whatever against the theory.

Note on Sir William Thomson's Arguments for the Age of the Earth.

Sir William Thomson has endeavoured to prove the recent age of the earth by three well-known arguments of a purely physical nature:—The first is based on the age of the sun's heat; the second, on the tidal retardation of the earth's rotation; and the third, on the secular cooling of the earth.

Argument from the Age of the Sun's Heat.—It will be obvious

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from the frightful conflagration which would ensue, as one star; or, thirdly, they would brush against one another, but not to the extent of preventing the stars from getting clear again." In the latter case he considers a double star is formed. Mr. Stoney's paper, though read in 1867, was not published till 1869.

Mr. Herbert Spencer, in his "First Principles" (pp. 532 to 535), has also directed attention to the fact that the stars distributed through space must tend, under the influence of gravity, to concentrate and become locally aggregated. Separate aggregations will be drawn towards one another, and ultimately coalesce. The result will be that the heat evolved by such collisions taking place under the enormous velocities acquired by gravity must have the effect of dissipating the matter of which they are composed into the gaseous state.

Both Mr. Stoney and Mr. Spencer consider the motions of the cosmic masses to be due wholly to gravity, but, as we have seen, gravity alone cannot account for the enormous amount of energy originally possessed by the sun.

that, if what has already been advanced in regard to the origin of the sun's heat be correct, it will follow that the argument for the recent age of the earth, based upon the assumption that the sun could have derived its store of heat only from the condensation of its mass, must be wholly abandoned, and that, in so far as this argument is concerned, there is no known limit to the amount of heat which the sun may have possessed, or to the time during which it may have illuminated the earth.

Argument from Tidal Retardation.—It is well known that, owing to tidal retardation, the rate of the earth's rotation is slowly diminishing; and it is therefore evident that if we go back for many millions of years, we reach a period when the earth must have been rotating much faster than now. Sir William's argument is,* that had the earth solidified several hundred millions of years ago, the flattening at the Poles and the bulging at the Equator would have been much greater than we find them to be. Therefore, because the earth is so little flattened, it must have been rotating, when it became solid, at very nearly the same rate as at present. And as the rate of rotation is becoming slower and slower, it cannot be so many millions of years back since solidification took place. A few years ago I ventured to point out† what appeared to be a very obvious objection to this argument, viz., that the influence of sub-aërial denudation in altering the form of the earth had been entirely overlooked. It has been proved, as we have seen, that the rocky surface of our globe is being lowered, on an average, by sub-aërial denudation, at the rate of about 1 foot in 6000 years. It follows as a consequence, from the loss of centrifugal force resulting from the retardation of the earth's rotation occasioned by the friction of the tidal wave, that the sea-level must be slowly sinking at the Equator and rising at the Poles. This, of course, tends to protect the polar regions and expose equatorial regions to sub-aërial denudation. Now it is perfectly obvious that unless the sea-level at the Equator has, in consequence of tidal retardation, been sinking during past ages at a greater rate than 1 foot in 6000 years, it is physically impossible the form of our globe could have been very much different from what it is at present, whatever may have been its form when it consolidated, because sub-aërial denudation would have lowered the Equator as rapidly as the sea sank. But in equatorial regions the rate of denudation is no doubt much greater than 1 foot in 6000 years, because there the

* Trans. Geol. Soc. of Glasgow, vol. iii., p. 1.

† Nature, August 21, 1872. Climate and Time, p. 335.

rainfall is greater than in the temperate regions. It has been shown that the rate at which a country is being lowered by subaërial denudation is mainly determined not so much by the character of its rocks as by the sediment-carrying power of its river systems. Consequently, other things being equal, the greater the rainfall the greater will be the rate of denudation. We know that the basin of the Ganges, for example, is being lowered by denudation at the rate of about 1 foot in 2300 years; and this is probably not very far from the average rate at which the equatorial regions are being denuded. It is therefore evident that sub-aërial denudation is lowering the Equator as rapidly as the sea-level is sinking from loss of rotation, and that consequently we cannot infer from the present form of our globe what was its form when it solidified. In as far as tidal retardation can show to the contrary, its form, when solidification took place, may have been as oblate as that of the planet Jupiter. There is another circumstance which must be taken into account. The lowering of the Equator, by the transference of materials from the Equator to the higher latitudes, must tend to increase the rate of rotation, or, more properly, it must tend to lessen the rate of tidal retardation.

The argument may be shown to be inconclusive from another consideration. The question as to whether the earth's axis of rotation could ever have changed to such an extent as to have affected the climate of the Poles is at present exciting a good deal of attention. The subject has recently been investigated with great care by Professor Haughton,* Mr. George Darwin,† the Rev. J. F. Twisden,‡ and others, and the general result arrived at may be expressed in the words of Mr. G. Darwin :—" If the earth be quite rigid no re-distribution of matter in new continents could ever have caused the deviation of the Pole from its present position to exceed the limit of about 3°."

Mr. Darwin has shown that, in order to produce a displacement of the Pole to the extent of only 1° 46′, an area equal to one-twentieth of the entire surface of the globe would have to be elevated to the height of two miles. The entire continent of Europe elevated two miles would not deflect the Pole much over half a degree. Assuming the mean elevation of the continents of Europe and Asia to be 1000 feet, Prof. Houghton calculates that their removal would displace the Pole only 199·4 miles.

* Proc. Roy. Soc., vol. xxvi., p. 51.
† Proc. Roy. Soc., vol. xxv., p. 328.
‡ Paper read before the Geological Society, February 21st, 1877.

It may now be admitted as settled that if the earth be perfectly rigid the climate of our globe could never possibly have been affected by any change in the axis of rotation. But it is maintained that if the earth can yield as a whole, so as to adapt its form to a new axis of rotation, the effects may be cumulative, and that a displacement of the Pole as much as 10° or 15° is possible.*

But then if the earth be able to adapt its form to a *change in the axis of rotation*, there is no reason why it may not be able to adapt its form to a *change in the rate of rotation*, and, if so, the flattening at the Poles and the bulging at the Equator would diminish as the rate of rotation diminished, even supposing there were no denudation going on.

Argument from the Secular Cooling of the Earth.—The earth, like the sun, is a body in the process of cooling, and it is evident that if we go back sufficiently far we shall reach a period when it was in a molten condition. Calculating by means of Fourier's mathematical theory of the conductivity of heat, Sir William Thomson has endeavoured to determine how many years must have elapsed since solidification of the earth's crust may have taken place. This argument is undoubtedly the most reliable of the three. Nevertheless, the data on the subject are yet very imperfect, so that no definite and trustworthy result can be arrived at by this means as to the actual age of the earth. In fact this is obvious from the very wide limits assigned by him within which solidification probably took place. "We must," quoting Sir William's own words on the subject, "allow very wide limits on such an estimate as I have attempted to make; but I think we may, with much probability, say that the consolidation cannot have taken place less than 20,000,000 years ago, or we should have more underground heat than

* A displacement of the Pole of less than 15° or 20° would be of very little service in accounting for the warm climate of Greenland during the Miocene and other periods. But a displacement to that extent, even supposing we admit the earth to be yielding, demands a condition of things which few geologists would be willing to grant. When it becomes generally recognised to what an enormous extent the temperature of the Arctic regions is dependent upon ocean currents, the difficulties in understanding how those regions have once enjoyed a temperate climate will disappear. Were the ice removed from Greenland that region would at present enjoy a warm summer, suitable for plant and animal life. It is the presence of ice rather than a positive deficiency of heat that makes Greenland so cold and barren (*see* Climate and Time, Chap. IV.). An increase in the quantity of heat conveyed by ocean-currents, merely sufficient to prevent the accumulation of ice, would completely transform the climate of the Arctic lands. And such an increase would take place during an Inter-glacial period when the eccentricity of the earth's orbit was at a high value and the winter solstice in perihelion,

we actually have,—nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that Leibnitz's epoch of 'emergence' of the 'consistentur status' was probably within these dates."*

III. THE GLACIAL PERIOD IN THE SOUTHERN HEMISPHERE.

By Thomas Belt, F.G.S.

THE tablets on which the ice of the Glacial period left its record in the southern hemisphere are probably now mostly covered by the sea, and we cannot trace its progress and extent with the same facility and certainty as in the northern temperate regions. Yet notwithstanding this, and also that the land surfaces of the South that have been glaciated have not been studied to anything like the same extent as in Europe and North America, points of resemblance are apparent, and grounds exist for the belief that both hemispheres have passed through a somewhat similar glacial experience.

In our hemisphere, I have sought to show in former papers, there were two ways in which the ice spread. One was an accumulation on mountain-chains, and a radiation from them over the surrounding country. The other, and I think by far the most important, was the gradual advance of a ridge of ice down the bed of the North Atlantic, and probably also of the North Pacific, which blocked up the drainage of the continents as far as it extended, and caused enormous lakes of fresh or brackish water and immense destruction of life amongst the animals that were caught on the plains by the rising floods. The marks left by the Atlantic ice are seen in Europe, as far as the southern extremity of Ireland, and in America, to the south of New York, and beyond these points, its further progress can be traced by the evidence of the interruption of the drainage of the continents as far as the northern slopes of the Pyrenees on one side of the Atlantic, and to the coasts of

* Trans. Roy. Soc. of Edinburgh, vol. xxiii., p. 161.

Virginia on the other. The real markings on the rocks, however, have not been traced in Europe farther south than 51° N. lat., and in America than 40° N. lat., excepting those of the ice that proceeded from the mountain ranges.

In the other hemisphere, within the distance from the Southern Pole that the ice has been shown by actual markings on the rocks to have reached from the North Pole on the European coasts,—that is, to latitude 51° N.,—there are no large masses of land, excepting the extreme end of South America and the Antarctic continent. And if we take a corresponding circle in the south to the limit the ice has left its marks in America or to lat. 40°, we shall still only embrace Patagonia in South America and the Middle Island in New Zealand. Even if we take the furthest limit of the extension of the Atlantic ice, as shown by its interference with the drainage of the American continent, we only bring South America as far north as the Rio Plata, and New Zealand and Tasmania, with the southern end of Australia, within the area where we could expect to find any similar evidence on the supposition that in the Glacial period the ice extended everywhere as far from the Southern Pole as its extreme limit reached from the Northern.

The conditions are therefore very different in the two hemispheres. In the one, broad continents stretch from within the Arctic circle toward and up to the Equator; in the other, nearly the whole of the temperate zone is covered with water. If, then, there were much less evidence than there is of the glaciation of southern lands, we need not have been surprised; but of late there seems to have arisen an idea amongst some geologists that there is no evidence in the southern hemisphere of the occurrence of a Glacial period, and it may be useful if I bring together what has been described, and show how far the phenomena agree with those of the northern hemisphere.

Commencing in America, immediately south of the Equator, we have first to deal with the remarkable theory of Agassiz, that the great valley of the Amazon was once filled with ice flowing from the distant Andes, which left an enormous terminal moraine on the Atlantic coast. This moraine he supposed blocked up for a time the waters of the great valley, and caused the deposition of various stratified deposits covered by a peculiar drift clay that rarely contains transported boulders. I have examined this deposit from Pernambuco northwards through the provinces of Ceara and Maranham, as far as Para. In some parts it is composed of small angular fragments of rock, cemented together

ochraceous clay, and resembling the brecciæ at the base of the Permian rocks in Westmoreland. In others it is a ferruginous clay, containing few stones, but with a layer of quartz pebbles at its base. It is not confined to the valleys, but wraps over the hills like a mantle. Now and then it contains large boulders of granite, but I could never satisfy myself that these might not have been left during the decomposition and denudation of the rocks of the neighbourhood. It differs much from any glacial deposit I have seen, and I am sorry that I can neither suggest any theory to account for its origin nor agree with that of the illustrious Agassiz. To the latter there seems to me insuperable objections. There are no moraines in the valley of the Lower Amazon. The terminal one might have been, as Agassiz suggests, washed away by the waves of the Atlantic, but the great glacier ought to have left others to mark the various stages of its recession. No remnant of these has been found, and we cannot believe that a glacier that left a huge moraine stretching for hundreds of miles across the whole seaward front of the Amazon Valley should have shrunk back for more than 1000 miles without leaving any whatever to mark its retreating course. The Valley of the Amazon abounds with birds and beasts, many of which are found nowhere else. Peculiar species of fishes swarm in the river and its tributaries. If the great valley was filled with ice, where did these find a refuge? To Agassiz this did not present any difficulty, as he believed that the present inhabitants of the world had been created since the Glacial period; but to those who hold the opinion that they are descended from pre-glacial ancestors, and that since the Glacial period there has not been much variation, the peculiar genera and species of the fauna of the Amazon Valley present serious objections to the theory.

Had Agassiz found in the Valley of the Amazon what he considered moraines, I should have had much difficulty in believing that he was mistaken, for no man had more experience of ice-action, present and past. Before him, Charpentier and others had worked out the conclusion that the glaciers of the Alps had in former days stretched far beyond their present limits, but they referred that extension to an elevation of the mountains, and not to a change of climate that affected all the northern parts of the continent. Agassiz accepted the theory, at first, that the upheaval of the Alps must, in some way or other, have been connected with the phenomena, but further study led him to abandon it, and conclude that the climatic conditions could not have

been local, but must have been cosmic. "When," he says, "Switzerland was bridged across from range to range, by a mass of ice stretching southward into Lombardy and Tuscany, northward into France and Burgundy, the rest of Europe could not have remained unaffected by the causes which induced this state of things."* Agassiz was thus the founder of the theory of a "Glacial Period," and in Great Britain, in Scandinavia, and in North America, evidence was soon found, and has been ever since accumulating, attesting the truth of his grand generalisation.

He was not likely to be mistaken as to what constituted a moraine, but he found none in the great valley, and he says himself that he had not here the positive evidence that had guided him in his previous glacial investigations.† Not so, however, with regard to his discovery of the marks of glaciation on the mountains of Ceara. Here, only about three degrees south of the Equator, he found undoubted moraines blocking up the valleys, and the evidence of glacial action was to him as clear as in the valleys of Switzerland, of Scotland, and of the Northern States of America.‡ This is the nearest point to the Equator at which glacial moraines have been found. On the other side of the line I found huge moraines in the northern part of Nicaragua, near the boundary between it and Honduras, in lat. 13° 47′ N.‖ This is the farthest south that glacial action has been traced in the northern hemisphere. Prof. W. M. Gabb has informed me that in his geological researches in the mountains of Costa Rica he found no evidence of glaciation. Between the moraines of northern Nicaragua and those of the mountains of Ceara there is an area comprising about 17° of latitude, and including most of Nicaragua, the whole of Costa Rica, of Columbia, of Equador, and of the great valley of the Amazons, in which no certain signs of glaciation have been seen. This wide region includes several large zoological sub-provinces, characterised by highly peculiar tropical genera, and a wealth of species not met with elsewhere on the continents to the north and south. Within this area are large groups of insects the extreme forms of which are linked together by a series of gradations, and every district has its representative forms of types that run through the whole. But as we travel south from the unglaciated districts lying between lat. 14° N. and lat. 3° S. the

* Geological Sketches, Second Series, p. 29.
† *Ibid.*, p. 207.
‡ A Journey in Brazil, by Mrs. Agassiz, p. 456.
‖ Naturalist in Nicaragua, p. 260.

species become more and more separated; the genera comprise species as far removed from each other as before, but these are not connected by the same intermediate forms. These gaps become wider and wider as we get farther south, until at the extreme end of the continent, where the rocks are glaciated from ocean to ocean, isolation, and not affinity, is the characteristic of the fauna, which is made up of waifs and strays that appear rather to have struggled in from the outside, upon a country that had been depopulated, than to have been developed within it.

In the glaciers of Ceara and northern Nicaragua we find the first parallel between the glaciation of the two hemispheres; and it is to be noted here, as it may be elsewhere, that there appears to have been more ice heaped up on the southern half of the world than on the northern, the much nearer approach of the glaciers to the Equator in Brazil than in Central America being the first evidence of it. This was probably due, not to greater cold, but to greater precipitation, proportional to the vast evaporating area of the Southern Ocean.

Travelling southward, we find that Mr. David Forbes noticed in Bolivia great accumulations of *detritus* with grooved stones and deeply-furrowed rocks, resembling those that he was familiar with in Norway.* And on the opposite side of the continent, near Rio Janeiro, Prof. Hartt has described moraines left by glaciers that formerly came down from the mountains of Tijuca.†

Still farther south the evidences of glacial action increase. For the description of the phenomena of La Plata, Patagonia, and Chile we must still turn to the observations of Darwin made more than forty years ago, which, when we consider that the glacial theory was in its infancy, evince the same rare powers of acute observation and philosophical generalisation that have since made his name so famous.

Ascending the River Santa Cruz, Darwin found, at a distance of about 100 miles from the Atlantic and at a height of about 1400 feet above the level of the sea, a great abundance of large angular boulders that had been transported from the Cordillera, the nearest slope of which was still about 60 miles distant. On both sides of the continent from lat. 41° S. to the southern extremity of it, he considers there is the clearest evidence of former glacial action in numerous immense boulders transported far from their parent source.‡

* Darwin, Origin of Species, Sixth Edition, p. 335.
† Geology and Physical Geography of Brazil, p. 26.
‡ Origin of Species, Sixth Edition, p. 335.

He describes Terra del Fuego as largely covered with till and boulder clay perfectly unstratified, and in which he looked in vain for marine remains.

On the western coast the ice from the Chilian Cordillera appears to have flowed across the Straits to the island of Chiloe, as Darwin found it covered with large boulders of granite and syenite that had come from the mainland. On one of the Chonos Islands he found a quantity of comminuted marine shells in the drift, and in Chiloe two or three fragments of a *Cytherea*.* We may compare these facts with those in the northern hemisphere, where the drift in the southern shores of the Baltic, brought by land ice from Scandinavia, is found—as in the vicinity of Bromberg—to contain broken and worn sea-shells that have probably been picked up in the passage of the ice across the ocean-bed.

Bearing just the same relation to the Glacial period in South America as the loess of the Rhine and the Danube does in Europe, and still more resembling the diluvial clay of the South of Russia, the Pampean mud is spread out in Rio Plata, Banda Oriental, and Entre Rios. It covers an immense tract of country. Darwin passed continuously over it from the Rio Colorado to St. Fé Bajada, a distance of 500 geographical miles, whilst M. d'Orbigny traced it for 250 miles farther north. From east to west, in the latitude of the Plata, it extends for at least 300 miles.

It is a reddish, slightly indurated, argillaceous earth, sometimes more than 100 feet thick, with lines of calcareous concretions, and occasionally changing into a compact marly rock. Marine shells are found scattered over the surface of this deposit at some places; but Darwin notices, as a remarkable fact, their absence throughout the deposit, excepting in the uppermost layers near Buenos Ayres. Even microscopical organisms appear to be very rare, and only found in the lower part of the deposit. Thus, in some of the red mud scraped from the tooth of a Mastodon found at the bottom of the Pampean mud at Gorodona, Prof. Ehrenberg found seven species of *Polygastrica* and thirteen species of *Phytolitharia*. Of these nearly all are of fresh-water origin, only three being marine. At Monte Hermoso, in Bahia Blanca, the lower part of the Pampean mud contains a similar assemblage of microscopical organisms. Prof. Ehrenberg considers that they must have lived in brackish water.

The most remarkable facts respecting the Pampean

* Trans. Geol. Soc., vol. vi., p. 426.

formation are its vast extent and the great number of the remains of large extinct mammals imbedded in it. Mr. Darwin says that he is firmly convinced that it would be impossible to cut a deep trench in any line across the Pampas without meeting with the remains of some quadruped. Wherever sections are exposed skeletons or detached bones are found, so that "the whole area of the Pampas is one wide sepulchre of these extinct gigantic quadrupeds."* At Punta Alta, within an area of about 200 yards square, were found the remains of no less than nine species of gigantic extinct mammals, including three Megatheriums, a Megalonyx, a Mylodon, and an extinct horse. Some of the skeletons were nearly perfect, besides which there were many detached bones. On the banks of the Parana two entire skeletons of the Mastodon were found near the base of the Pampean mud.

The Pampas extend southwards to the Rio Colorado. There, beds of gravel begin to take the place of the Pampean mud. These are at first thin, and composed of small pebbles. Farther south the gravel-beds are composed of coarser pebbles, with sometimes large boulders. This deposit of shingle extends for 800 miles up to the Straits of Magellan. In some parts the plains of gravel rise in step-formed terraces to a height of 1200 feet above the sea. Marine shells are scattered over the surface of the plains, but are not found in the beds of gravel, which, like the Pampean mud, contain in some parts numerous remains of the great extinct quadrupeds.

Darwin mentions that the Mammalian remains have not anywhere been found in existing marshes or peat-beds: all are entombed in the body of the deposit of which the plains are composed.

In the Pampas, in the caves of Brazil, and in deposits of similar age in other parts of South America, more than one hundred extinct species of quadrupeds, many of great size, have been found. There are representatives of the sloths and armadillos as large as existing elephants. There are also representatives of genera that do not now exist in South America, but still live in other parts of the world—such as leopards and antelopes. The remains of the horse are abundant, though when the Spaniards discovered America it was extinct there.

Mr. Wallace, in his great work on the distribution of

* Darwin, Naturalist's Voyage, p. 155. The whole of my information respecting the Pampas is derived from this work and from The Geological Observations on South America by the same author.

animals, has given an excellent summary of the facts known respecting these lost animals, and has placed the question of their extinction before us with clear-cut distinctness.* He shows that in very recent geological times a great change has taken place in the fauna not only of South America, but of North America and Europe, and that this change is unprecedented in older geological periods. We now live, he says, "in a zoologically impoverished world, from which all the hugest and fiercest and strangest forms have disappeared." He urges, with luminous force, that there must have been some physical cause for this great change, which must have acted at the same time over large portions of the earth's surface. Such a cause he considers is to be found in that great era, the glacial period.

There is much to be said in favour of Mr. Wallace's conclusion. The Pampean mud occupies the same relation to recent deposits as other clays whose glacial age has been satisfactorily determined. Both it and the gravel-beds of Patagonia, in many places, lie directly above marine deposits of undoubted late Pliocene age, as they contain shells of *Ostrea Patagonica*. They hold, therefore, the same position between the tertiary and recent deposits that glacial beds occupy in other countries. The Patagonian deposits also contain large boulders that have been transported from the Cordillera.

The Pampean mud has been compared with the loess of the Rhine—a similar fine slightly-indurated clay, which contains the bones of the extinct mammoth and woolly rhinoceros. It appears to be still more like the beds of diluvial clay that form the wide-spread steppes of Southern Russia. This clay is of undoubted Glacial age, and when traced northward contains large blocks of stone derived from rocks hundreds of miles to the north, just as the Pampean mud when traced southward is replaced by gravel-beds with boulders from the Cordillera. In the January number of this Journal I have advocated the theory that the diluvial clays of Northern Europe and the loess of the Rhine and the Danube were spread out in a great lake, when the bed of the Atlantic was occupied by ice that stopped the drainage of Europe as far as it extended. In eastern North America there is evidence of a similar interruption to the drainage of the continent to at least as far as North Virginia.

It is a remarkable fact that to about as far from the South Pole in South America as from the North Pole in North

* Geographical Distribution of Animals, vol. i., p. 143.

America there is the same evidence of the land having been covered with water which was not that of the sea; as the deposits left by it do not contain marine remains. If we may suppose that a similar mass of ice accumulated around, and spread from the Antarctic as from the Arctic regions, and that ultimately a prolongation or arm of that icy mass reached the eastern coast of South America, so as to dam back the streams as far as the Rio Plata, we shall have, I believe, a complete explanation of the deposition of the Pampean mud and the Patagonian gravels, and of the destruction of the great mammals and the entombment of their remains.

The present physical geography of the world and the existing distribution of its inhabitants were immediately preceded by a wide-spread entombment of land animals, unparalleled elsewhere in the geological record. In lake basins and estuaries of Eocene and Miocene age there have been preserved abundant remains of land animals, more especially in connection with the movements of elevation of certain mountain chains and the volcanic phenomena that accompanied them; but, compared with that which we are discussing, these entombments are insignificant and partial. The destruction of life that took place in the Glacial period was continental, if not world-wide, in its extent. The deposits in which the remains are found were not formed in old lake basins; they fringe, and in some cases nearly overspread, the continents, or run in great arms up the larger valleys. Here and there in America, Europe, and Asia, there are isolated deposits containing the remains of tertiary mammals, but the bones of the glacial mammals are spread over all the northern parts of the northern continents, and in South America are of equal extent.

The blockage of the coasts by ice flowing down the ocean beds seems just such an event as would bring about the destruction of life and the entombment of the remains of which we have so much evidence. I shall have to return to the consideration of the possibility of such a mass of ice having advanced northwards from the Antarctic circle when we have examined the evidences of glaciation in New Zealand; but let us assume now for a moment that such an accumulation did take place, and that it flowed down the eastern coast of South America; then, as it blocked up the drainage of each great valley, progressively from the south, northward, the waters pounded back would rise and overwhelm the animals living on the plains. The nature of the deposits favours this supposition. The lowest parts, or

those first formed, contain some evidence of brackish water conditions such as might have been produced by the embracement of the salt water of bays within the area blocked up by the ice; but the upper parts contain nothing but the remains of land animals, whilst the proof of the presence of water is complete in the rounded and stratified shingle forming the Patagonian plains.

Mr. Wallace has shown that the cave fauna in Brazil differs from that of the Pampas in containing a larger proportion of existing genera, and is inclined to believe that this may imply that the Pampean mud is a little older than the cave deposits.* It may, however, be due, I think, to the destruction of life having been more complete in Buenos Ayres and Patagonia than in Brazil, and that, in pre-glacial times, as now, the former countries formed a part of a distinct zoological sub-region. That the destruction of life in Brazil should have been less complete than farther south is likely—not only because we have no proof that the great flood extended so far north, but because there was a large extent of country, neither covered with ice nor overwhelmed with water, where many of the species might be preserved. In Patagonia and Buenos Ayres the lower country was covered with water, the upper mostly with glaciers descending from the mountains, so that the retreat of the animals from the flood was in a great measure cut off. The peculiar species that have been preserved in the Chilian sub-region, which includes Patagonia and Buenos Ayres, are principally alpine forms, such as the Chinchillas, the Alpacas, and the Viscachas, which we may suppose found a refuge during the great flood in some high lands not covered with glacier ice. Others may have retreated northward up the western side of the Andes, and returned southward after the greatest severity of the Glacial period had passed away. The remains of an extinct llama are found on the high plains of Mexico, so that the genus had undoubtedly a former greater extension northwards. The principal extirpation of life appears to have been amongst the bulky species, on whom the changes of environment would press most heavily, and which were confined to low-lying districts. Thus the Mastodon, whose remains are found at great heights in the Andes, appears to have escaped the complete destruction that befel its bulky companions at the time of the greatest extension of the ice, as I saw remains of it in Mexico that from their fresh appearance were probably as recent as those

* Geographical Distribution of Animals, vol. i., p. 146.

of the species that is found in post-glacial deposits in the Northern States.

Whilst the eastern coasts of Patagonia are fringed with plains of gravel, the western are indented with deep fiords like those of Norway, and, as Dana and Ramsay have shown, these fiords are evidence of great glaciation, and have probably been excavated by ice-action.

Leaving South America and passing to the continent of Africa, we find but little evidence of the Glacial period, as its southern extremity only reaches to about lat. 35° S. Mr. G. W. Stow has, however, shown that the southern ranges of mountains undoubtedly bore glaciers. Thus the Katberg range is in many parts rounded and smoothed, as if by the passage of ice, and on its northern slopes there are great mounds and ridges of unstratified clay packed with angular boulders of every size, from small gravel and pieces of a few pounds weight to masses of several tons.* The slopes of the Stormberg are similarly glaciated, and there are immense accumulations of morainic matter in all the valleys. Both lateral and terminal moraines have been observed in these mountains. In British Kaffraria, near Greytown, the ice would appear to have reached nearly to the present sea-level. Mr. Stow comes to the conclusion that the rounding-off of the hills in the interior, the numerous dome-shaped rocks, the enormous erratic boulders in positions where water could not have carried them, the frequency of unstratified clays, clays with imbedded angular boulders, drift and lofty mounds of boulders, and the large tracts of country thickly spread over with unstratified clays and superimposed fragments of rock,—all indicate the conditions that in other countries characterise the Glacial period.†

The Rev. W. B. Clarke has informed Mr. Darwin of facts from which it appears that there are traces of former glacial action on the mountains of the south-eastern corner of Australia, but I do not know the particulars.‡ I have not seen any notice of glacial phenomena in Tasmania, but it is extremely probable that they will be found. In New Zealand the evidences of former glaciation are clear and unmistakable, and they have been described by many able observers. The large glaciers that still exist in the mountains of the South Island bear about the same relation to the

* Quart. Journ. Geol. Soc., vol. xxvii., p. 539.
† *Ibid.*, p. 544.
‡ Darwin, Origin of Species, Sixth Edition, p. 335.

ancient ones as those of the European Alps do to the much larger ones of the Glacial period. The western sea-board is penetrated by long sounds or fiords, many of which are more than 1000 feet deep. On the eastern side of the range the fiords are replaced by arms of large and deep fresh-water lakes; and so far do the marine fiords on one side, and the arms of the fresh-water lakes on the other, cut into the mountain chain that in many places they reach to within 10 miles of each other.* The New Zealand geologists appear to be unanimous in ascribing the formation of the sounds and deep lakes to the action of ice. The former great extension of the glaciers is marked by immense morainic accumulations and transported boulders. These deposits have been ably described by Dr. Haast. For more than 90 miles south of the River Mikonuhi, on the western coast, all the lower country is covered by mounds and sheets composed of unstratified clays packed with boulders from the mountain chain. The moraines form low hills bounding the sea, the waves of which have cut them into cliffs. Along the whole of this shore glaciers must have descended from the New Zealand Alps down to, and probably beyond the present sea-level. Some of the imbedded blocks exposed in the sections on the sea-coast are of immense size, often larger than the celebrated Pierre-a-bot, in the Jura. These blocks have principally been brought from the very centre of the chain, and the distribution of the different rock formations in the drift and alluvial beds has led Dr. Haast to the conclusion that the configuration of the mountain chain was similar to what it is now at the time of the great extension of the glaciers.†

On the eastern side of the range the old glaciers did not reach to so low a level as on the western side, but terminated many miles from the sea-coast, which is bounded by great plains of stratified drift. In the northern part of the South Island Mr. Locke Travers has shown that many of the lakes are enclosed by huge moraines,‡ and in the North Island there is also evidence of intense glacial action.

The southern and eastern coasts of the South Island are bordered by great plains, composed of gravel and rounded boulders, which sometimes overlie beds of silt containing bones of the extinct moas. In the south part of the island the Southland Plains extend for nearly 40 miles along the

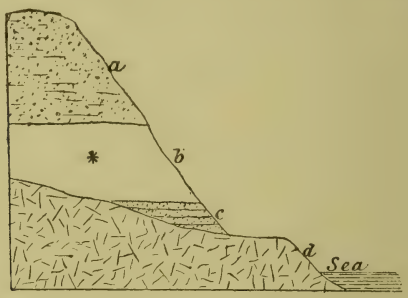
* Hutton and Ulrich, Report on the Geology and Gold-Fields of Otago. 1875.

† Quart. Journ. Geol. Soc., vol. xxiii., p. 350.

‡ *Ibid.*, vol. xxii., p. 254.

coast, and back from it for about 26 miles, besides sending arms up the valleys of the Mataura, Oreti, and Jacob Rivers. This plain is composed of shingle, sand, and clay, with some seams of lignite near the base of the deposits. It wraps around the Moonlight Range and Hokonui Hills, which stand up like islands out of it. The beds of which it is composed contain the bones of the great extinct apterous birds. Marine remains are not found, but Captain Hutton ascribes its formation to the action of the sea when the land stood at a lower level, as he finds it impossible to believe "that the alluvia of the Mataura, the Oreti, and the Jacob Rivers should all join one another with gradual slopes behind the seaward range of hills without the intervention of some uniform widely-acting cause, such as the sea."*

SECTION N. SIDE OF OAMARU (CAPTAIN HUTTON).



a. Gravels of the Plains.
b. Silt with Moa Bones at *.
c. Gravels with twenty-two species of Marine Shells, all but two still living on the New Zealand coasts.
d. Basalt.

These plains rise gradually from the sea-coast until they attain a height of about 600 feet next the mountains. On the coast they overlie marine deposits of late Pliocene age. The above section, from the work already quoted, shows the relation of the beds on the north side of Oamaru Cape.

The silt formation extends inland for a considerable distance, and is extensively developed at Hampden. Captain Hutton notes the great analogy it presents to the Pampean formation of South America. The remains of the extinct birds are distributed, like those of the extinct beasts at Buenos Ayres, throughout the deposit. The above section might indeed be almost exactly paralleled from South America, more especially in Patagonia, where the gravel-beds

* Geology and Gold-Fields of Otago, p. 79.

containing bones of extinct mammals overlie marine strata of late Pliocene age.

On the east coast, great plains, separated by rocky promontories, border the sea. The well-known Canterbury Plains are the greatest of these outspreads of gravel: they have been well described by Dr. Haast, and the question of their origin has been a fruitful source of controversy; they are 112 miles in length, and run back from the coast to the base of the mountains in the interior; gradually rising until they attain an elevation of about 1500 feet above the sea, according to Dr. Hochstetter.* These plains are composed of boulders, gravels, sand, and clay, and in some parts the deposits are upwards of 200 feet thick. Throughout the whole formation no marine remains have been found. At Banks's Peninsula the gravel-beds are replaced by deposits of silt that cover the volcanic hills to a height of about 800 feet above the sea. This fine loam must, from its description, closely resemble the loess of the Rhine and Danube, and it contains the bones of the great wingless birds and land-shells, but nowhere has the least trace of marine life been found in it.†

We have thus in New Zealand a repetition of the phenomena observed in Patagonia—great plains of gravel and silt on the eastern, and deep sounds or fiords on the western, coasts. In both countries the deposits of the plains entomb the remains of large extinct animals, and their geological age is fixed by both overlying marine beds of late Pliocene age. They are the representatives in position of the glacial beds of the northern hemisphere. Considering the very different distribution of land and water in the two hemispheres the resemblance of their glacial phenomena is most remarkable. Yet still more remarkable is it that most of the able naturalists of New Zealand not only deny that there is proof of a Glacial epoch in their country, but even are inclined to refuse us one elsewhere on the evidence they find there. I do not gather very clearly Dr. Haast's opinion on this question, and as a few years ago he was certainly in favour of New Zealand having been covered with an ice-sheet like Greenland, he may perhaps be excepted; but the remainder of the geologists, headed by Dr. Hector and Capt. Hutton, contend that the glaciation of New Zealand had no connection with a general glacial period, and that the great accumulation of ice was owing to the mountains

* New Zealand, English Edition, p. 508.

† Dr. Haast, Trans. New Zeal. Inst., vol. vi., p. 423.

having then been higher. Dr. Hector, who first suggested this explanation, candidly admits that the shore deposits do not support it, and considers that there must have been unequal movements of elevation, and that the former greater weight of the mountains was due to their greater bulk, which has since been reduced by denudation.

Dr. Hector, so far as I can make out from his writings, only seems to offer this as an alternative to accepting the occurrence of a general Glacial epoch, and we must turn to the works of Captain Hutton for the reasons that have weighed with the New Zealand geologists in rejecting the theory of a Glacial period, which, since Agassiz first propounded it for the northern hemisphere, has been adopted by nearly all our leading geologists. In the first place Captain Hutton thinks that the date of the last great glacier period of New Zealand, must be placed much further back than that of the northern hemisphere, because, since it occurred, some of the old lake basins have been filled up with deposits brought down by the streams; the surfaces of the rocks that were formerly covered with ice have been weathered to a depth of ten or twelve feet; old river courses have been filled with gravels, and the streams have cut new channels to great depths, so that in many places the drainage system of the country has been altered.* But in reality these facts should rather have been advanced as evidence of the analogy of the glacial phenomena of the two hemispheres instead of as a distinction between them. We have but to appeal to the erosion of the deep gorge of the Niagara, for at least a distance of three miles since the glaciation of Canada, to the reversal of the drainage of the great lakes from the Mississippi into the St. Lawrence, to the buried old river channels of the north of England and of Scotland, and to the numerous filled-up lake basins of Europe and America, to show that it is the resemblances and not the differences that strike us with greatest force.

Captain Hutton has not, however, restricted his argument to the physical side of the question, but has sought to strengthen it by appealing to the evidence of the organic remains. It is admitted that the Glacial deposits contain no marine organism, but, as we have already seen, they overlie beds of gravel and sand, with sea shells of late Pliocene age, as shown in the Oamaru section. These late Pliocene deposits are largely developed at Wanganui in Cook's Strait, and it is from a study of the present and past range of the species of the marine mollusks in these and similar beds,

* Geology and Gold Fields of Otago, p. 94.

that Captain Hutton's ingenious argument is founded. He has given a list of a great number of mollusks found in these beds that do not now live so far south, whilst only a few occur that do not now live so far north.* This, he thinks, proves that there was no reduction in the general temperature in times immediately preceding the glaciation of the country. We do not know, however, the age of these preglacial deposits, and they may be as far back in time as our coralline crag, and the value of the evidence has been much lessened by some facts described by Mr. C. W. Purnell. He states that at Wanganui there are three distinct fossiliferous strata, separated by thick beds of volcanic mud, and that the shells of these different horizons are mixed together in Captain Hutton's lists.† If this is so, it may yet appear, when the fauna of each zone is more critically studied, that the same evidence of a gradual refrigeration of the climate in the Pliocene epoch exists in New Zealand as in Europe.

Captain Hutton further argues that as many of the shells that extend back to the Miocene epoch, and still exist on the coast of New Zealand, are littoral species, and are not found elsewhere, we should have to suppose, if the extension of the glaciers was due to the change in the climate, that during the cold period they crossed the deep sea to Australia or Polynesia, and that, on the return of a warmer climate, they all came back again to New Zealand without leaving any behind on the coasts they had retired to during the Glacial period. He concludes, therefore, that since the Miocene period there can have been no reduction of temperature sufficient to account for the former extension of the glaciers, and that we must necessarily look to elevation of the land as the main cause. He admits, however, that it is possible that the two may have been combined, but considers that at present the evidence seems to be in favour of there never having been a Glacial epoch in New Zealand, and consequently none in the southern hemisphere.‡

Now, if the theories I have advocated respecting the Glacial period are correct, the two conditions of the glaciation of the land, and at the same time its relatively greater elevation above the level of the ocean, must have existed together. For such an amount of ice as is necessary could not be piled up around and outside the Arctic and Antarctic circles at the same time without abstracting so much water

* Trans. New Zeal. Inst., vol. viii., p. 383. Captain Hutton has also published an abstract of his views in the Geological Magazine, 1875, p. 580.

† Trans. New Zeal. Inst., vol. vii., p. 453.

‡ Ibid., vol. viii., p. 387.

from the ocean as to lower its level to the extent, I believe, of about 2000 feet; and this lowering of the ocean would give a coast line along which the littoral mollusks might retreat northwards during the Glacial period. In the line of soundings made between Australia and New Zealand by the officers of the *Challenger*, it was found that from the Australian shore the water deepened gradually until a depth was attained of 2600 fathoms, at about one-third the distance sccross. Nearer New Zealand the water shoaled suddenly, and at a distance of about 280 miles from its shore, a depth of only 275 fathoms, or 1650 feet, was found with a rocky bottom. These soundings were taken in one line only, and there can be scarcely a doubt that a lowering of the sea level 2000 feet would add more than 300 miles to the extension of New Zealand northward, and afford the necessary refuge for its fauna and flora during the Glacial period.

To prove the necessity of lands to the northward, now submerged, having then been above the sea, it was not necessary to appeal to the evidence of the marine shells, which is so far doubtful, as it is known that many shallow water species, under other circumstances, live at a great depth ; as that of the inhabitants of the land was equally available and more certain. For the bones of the great extinct birds are not confined to the drift beds, but are found in old kitchen middens, ovens, encampments, and other places on the surface of the glacial beds, proving, that after the cold period had passed away, they returned again. It is certain that they must have been expelled from the existing area of the South Island, when glaciers came down beyond the present coast lines on the west side, and the great plains of drift were being formed under water on the east. The marine mollusks might have passed across some channels of the sea, but the apterous birds required a continuous land passage during their retreat, and the withdrawal of the waters of the ocean would have afforded this, not only to the North Island, but to the great plateau now submerged, whose existence has been indicated by the few soundings of the *Challenger* expedition.

We may agree with the New Zealand geologists that their country stood at a higher level above the ocean than it now does when it was glaciated ; but instead of looking upon this as evidence in favour of local causes having led to the accumulation of the ice, it is only another link in the evidence to prove that the level of the ocean was lowered, and that the glaciation of the southern hemisphere took place at

the same time as that of the northern, as it occurred when the land stood relatively to the sea higher than it now does. There is the same objection to its being movements of the land that effected this, as there is to the theory of similar land movements in Great Britain, for the New Zealand geologists would send their country 3000 feet up into the air to allow the glaciers to descend to the present sea-level, and lower it 2000 feet to permit the gravels of the plains to be spread out, making a total movement of more than 5000 feet; yet after these enormous imaginary oscillations, we find everywhere, as we do in England, the pre-glacial shell beds with their littoral species only within a a few feet of the present shore line.

Let us now turn to the consideration of the question of the origin of the great plains of drift and silt that border the eastern and southern coasts. Dr. Haast, in his able essay on the structure and origin of the Canterbury Plains, considered that the gravels were the exuviæ from the great glaciers of the interior that had been spread out by the floods produced whilst it was melting. Some serious objections have been urged against this view. The sheets of gravel wrap around the hills, and are spread right across the water-sheds between different river systems. They are nearly level for scores, or even hundreds of miles. Ranges of hills are isolated so that they rise up from the plains like islands out of the sea. The waters, necessary to overflow such an extent of country, would be raging torrents, which, instead of depositing sediments, would sweep everything before them into the ocean; and would be rather agents of destruction and denudation than of deposition above the sea-level. There are beds of silt up to 800 feet above the sea-level at Banks's Peninsula, and for the deposition of these beds we require the presence of tranquil waters, and not torrential floods. It may also be remarked that it is not probable that, even on the hottest day, sufficient ice could be melted to produce the quantity of water required to submerge the country next the sea shore beneath a flood several hundred feet deep. The very statement of the enormous quantity of water required seems to condemn the theory when we remember that every day the water produced would be carried off, if near the sea there was nothing to stop its outflow and dam it back.

These considerations have led the generality of New Zealand geologists to adopt the theory that the great seaward plains are of marine formation, and that the gravels, sands, and loams were spread out when the land stood much

lower than now. The theory is the same as has been proposed to account for the similar outspreads of sheets of gravel and drift in other parts of the world in the Glacial period, and the same objection is to be made to it—the absence of marine remains. In the case of New Zealand, the drift has not been found to contain even the broken and worn sea shells that in Europe occur in some cases where ice had advanced on the land from the sea, bringing some of the productions of the latter with it.

If the theory is correct, the land must have sunk nearly 2000 feet, to allow of the formation of the Canterbury Plains. As it was slowly sinking and rising again, every portion of the area submerged would become a sea beach, and pass through the different stages of shallow and deep water; that at the present sea-level to a depth of about 2000 feet. As the land emerged, these stages would again be passed through. Along these successive coast-lines there would be bays and promontories, estuaries and sand banks, rocky cliffs and pebbly beaches. We have evidence, as Captain Hutton has shown to us, that the mollusks never left the shores. What then has become of their remains? We may grant that there must have been much destruction of the old sea beds during the rise of the land, but there ought to be some exceptions from that devastation, and not total and supreme annihilation. The beds themselves have not been destroyed; there are sheets of gravels, sands, and clays, lying as they are supposed to have been spread out during the submergence, but they contain no marine organism. Contrast these with the pre-glacial beds in the same districts, as, for instance, the clays and sands on the coast at Wanganui and Oamaru. These teem with sea shells, yet the unconsolidated sands and gravels of which they consist have passed through all the vicissitudes of the Glacial period. In Europe it is the same; the pre-glacial marine beds at Cromer and elsewhere, often of loose sand, are full of marine shells. We have also in Europe the post-glacial raised beaches, as in Scotland, up to 50 feet, and in Norway up to 600 feet above the sea-level, and these are often so crowded with marine shells, that in Norway they have been worked for many years for burning into lime.

We cannot believe that no life existed in these seas, for we know that it abounds both in the Arctic and Antarctic regions up to the foot of the great ice-sheet. Dr. Hooker states that along the shores of the Victoria Barrier the soundings were invariably charged with diatomaceous remains, that the water and the ice of the South

Polar ocean abound with them, and that they occur in such myriads that where washed on the bergs or pack-ice by the sea they stain it a pale ochreous colour. These remains were detected along every ice-bound shore, and in the depths of the adjoining ocean between 80 and 400 fathoms.* Nor is life in the polar regions restricted to these minute organisms, for recent Arctic explorations have proved that mollusks and crustaceans swarm at the very foot of the Greenland glaciers, and even to the north of Siberia where the water is freshened by the great floods, poured into the Arctic Ocean by the Obi and the Yenisei.

In New Zealand, Mr. Crawfurd has recognised the significance of the absence of marine life in the deposits forming the plains, and has suggested that they have been spread out in great fresh-water lakes. To account for the origin of these lakes he supposes that there was formerly a barrier of land running down the south-eastern coast, which has since entirely disappeared.† Agreeing as I do with Mr. Crawfurd, that the facts he has brought forward show that these deposits are not marine, it is difficult to believe in the possibility of a ridge of land having been elevated, since Pliocene times, along the whole coast of New Zealand, to a height of 2000 feet, and utterly destroyed since, so that not a vestige of it remains.

It will have been premised that I attribute the origin of these deposits, as I have those of similar ones in South America, to the advance of the Antarctic ice upon the coast so as to block up the drainage of the land. The great feature of the Glacial period is, of course, the enormous development and accretion of ice, and to call in its aid appears less hazardous than to invoke oscillations of the earth's crust, which, in our present ignorance of the condition of the interior, may be impossible. I do not hide from myself the vast quantity of ice that is required on this supposition; nor the difficulty of accounting for its formation, and I have only been driven very gradually and unwillingly to the conclusions I now hold respecting its extent. In my last paper in this Journal I endeavoured to show that in the North Atlantic area the ice would accumulate most at the northern end of that great evaporating basin; that when it was heaped up on Greenland to such a height as to intercept all the moisture of the currents of air travelling northwards, the precipitation would take place on its southern slope only; and

* Flora Antarctica, vol. ii., p. 503.
† Trans. New Zeal. Inst., vol. viii., p. 369.

that the latter would advance southward, not only by flowing glacier-like, but by the area of precipitation itself being moved southward. A similar accumulation and extension must, I think, have taken place in the Antarctic regions. We have learnt that there is a great precipitation of snow around the edge of the ice-sheet with which the Antarctic continent, so far as we know, is covered; but an equilibrium has been attained between the forces concerned in its formation on the one hand, and its liquefaction on the other, so that it does not now advance further.

In the Glacial period that equilibrium must have been disturbed in favour of the accumulation of the ice, not necessarily by greater cold, which would tend to lessen the precipitation, but from some cause favouring a larger amount of moisture reaching the polar regions, and falling there in the form of snow. If this did occur, I believe that the ice might obtain powers of accumulation and growth, where it was fed by large evaporating areas, that might enable the ice-sheets now nearly confined within the Arctic and Antarctic circles to advance upon and invade the temperate regions; especially down the ocean depressions. I indicated some of the causes of accretion in my last paper. The currents of air travelling from the tropical and temperate zones towards the Poles, such as the counter trade winds, are charged with moisture which is precipitated when they reach colder regions or encounter mountain-chains. Confining ourselves to the southern hemisphere we find that much of the moisture falls as rain in low latitudes, yet that a certain proportion of it reaches the Arctic circle, and there falls as snow, and recuperates the ever-melting ice-sheet. Let us suppose that some change took place by which much more of this moisture would reach the southern ice-sheet, such, for instance, as would probably be effected if the obliquity of the ecliptic was increased. This, by raising the temperature of the temperate zone, would cause much more moisture to reach the southern ice-cap, and be precipitated there so as to add to its extent and height. As the edge of the ice-cap moved northward, it would gather to itself not only the moisture that would have reached it in its old position, but that which belonged to the area it now occupied.

As I remarked in my last paper on the Atlantic Glacier, but reversing the direction of its progress, it would be a ridge of ice slowly advancing northward, and appropriating the precipitation of the regions it invaded to aid in its further progress. Its advance would be due to two causes,

the flow of the ice itself, and the area of frozen precipitation being moved northward by the accretion on the northern slope of the ice-sheet; for when it had attained a certain height above the sea-level it would intercept the whole of the moisture of currents of air travelling over it, and cause its precipitation on its northern slope, so that, as it advanced further from the Pole, there would be more and more precipitation, and its progress would only be ultimately checked by it reaching a warm enough latitude to greatly increase the liquefaction of the ice. The height of the ridge would increase as it reached more temperate regions, as it would primarily depend upon the altitude to which the moisture was carried before it was precipitated, and this would be greater the lower the latitude the ridge of ice attained to.

I do not suppose that this ridge of ice, or the zone of greatest frozen precipitation, ever reached to New Zealand or to the Rio Plata, but only that the ice flowing from it did so. The zone of greatest precipitation may have been hundreds of miles farther south, and if anyone then could have stood on the edge of the great ice-sheet he would have seen to the south probably what appeared a level plain of ice reaching to the horizon, so gradual would have been its rise in that direction. M. Favre, in his elaborate account of the old glaciers of the northern rivers of the Alps, has proved that at their greatest extent the slope of the upper surface of the ice was very small, and for great distances nearly horizontal.* And Mr. Helland, describing the great ice-sheet that covers North Greenland, states that it resembles a great sea, but seems to rise slowly inland. Where the glaciers are largest on the coast the rainfall is not considerable, and Mr. Helland concludes from this that the source of the ice must be far in the interior.† In consequence of this property of flowing with a small slope the ice has been conveyed to the coast for more than a hundred miles, at least, from where it was accumulated, and the whole of the lower part of its course is far below the limits of perpetual snow. The coast of Greenland does not, therefore, owe its icy mantle to the climate there, but to the outflow from the snow-gathering grounds of the far interior. And in the Glacial period the neighbourhood of Lyons was glaciated by ice that had flowed from the far distant Alps, and which in a great part of its course was nearly horizontal on its upper surface.

* Archives des Sciences de la Bibliothèque Universelle, 1876, t. lvii.
† Quart. Journ. Geol. Soc., vol. xxxiii., p. 146.

Judging from what we know of the slope of the upper surfaces of the present ice-sheets of Greenland and Spitzbergen, and from the marks left by the old Alpine glaciers, we shall not, I think, be justified in allowing the still larger circumpolar ice-sheets a greater surface slope than 1 in 200; and supposing that the ice was 2000 feet higher than the present level of the sea, when it reached New Zealand, we should have to go back in the direction from which it flowed more than 200 miles before we reached a height of 8000 feet above the present sea-level, which may have been the zone of greatest precipitation.

Prof. Tyndall, in his well-known and often-quoted observations on the Glacial period, has so forcibly and clearly defined some of the necessary conditions to allow of a great accumulation of ice, that I may be allowed again to refer to them. He shows that the ancient glaciers required for their formation that water should be evaporated by heat, and that by lessening the force of the sun's rays we should not increase the glaciers, but cut them off at their source; and he illustrates this by referring to a distilling apparatus, and reminding us that if we wished to increase the quantity distilled we should certainly not attain our object by reducing the fire under our boiler. "It is quite manifest," he says, "that the thing most needed to produce the glaciers is an improved condenser; we cannot afford to lose an iota of solar action; we need, if anything, more vapour, but we need a condenser so powerful that this vapour, instead of falling in liquid showers to the earth, shall be so far reduced in temperature as to descend in snow."* If the theory I am advocating is correct the great condenser that exists within the Antarctic circle moved northward, and as it so moved the vapour to be condensed increased in quantity, for the reasons I have already given. It was prevented from falling in liquid showers, though it did not reach the earth as snow, but as ice flowing from the icy ridge that was fed and capped with snow.

Air, as it rises, expands, and much of its heat becomes latent, so that it is cooled according to a definite and well-ascertained law. To speak popularly, there is an inexhaustible store of cold up above, available for glaciation, if it could only be brought down.† The vapour in the air is chilled when it rises to a great height through the abstraction of its heat by the expanding air. The vapour is thus condensed, and at high altitudes frozen, so that it falls as snow.

* Heat as a Mode of Motion, p. 188.

† I am indebted to Prof. Joseph Henry, of Washington, for this suggestion.

Most of this snow is again liquefied before it reaches the earth by the higher temperature of the lower portions of the atmosphere it has to pass through. But if it be intercepted above, on a mountain range, it may accumulate there and feed glaciers that descend for thousands of feet below the snow-line. In its descent, in this form, only its upper surface is exposed to the warm air, and the lower portions of the mass are preserved as in an ice-house. Thus, glaciers may be said to bring down the cold of the upper regions of the atmosphere, and the advance of a ridge of ice from the north and south into the temperate zones, in the Glacial period, would give us the machinery for doing this on a scale commensurate with the extent of the glaciation of the two hemispheres.

According to this theory, the ice that reached the coasts of New Zealand and South America was not due to a great lowering of the mean temperature of these countries, but flowed down upon them, bringing with it the cold of regions thousands of feet above the sea-level and hundreds of miles to the southward. The theories of local glaciation require a much greater change of climate than this does, just as in the valley of Chamoinix a very considerable lowering of the present mean temperature would be necessary to cause ice to accumulate there, instead of flowing down to it from Mont Blanc. In all the treatises on the Glacial period that I have seen, the question sought to be answered is—"What causes would bring about changes of climate to allow perennial snow to accumulate on lands in temperate regions. Instead of this, I think the problem to be solved is—What are the conditions that would cause ice to be piled up around the Arctic and Antarctic circles, and gradually invade the temperate zones, bringing down the cold of the upper regions of the atmosphere as it progressed? I shall not now attempt to answer this question, but occupy what space I have left in showing how such an advance of ice explains the facts we have been considering in the southern hemisphere and some others in this part of the world.

The advance of the ice from the Antarctic regions would not be directly southward. The moist winds that fed it blew from the north-west, and its progress would be to the south-east towards its source of supply. And as the quantity of moisture in the air would be greater in some regions than in others, owing to the irregular distribution of land and water to the northward, the progress of the ice would not be equal all around the Antarctic, but some parts would

be greatly in advance of others. In both New Zealand and Patagonia there are wide plains to the east and a coast indented with deep fiords to the west. It is certainly a curious circumstance that we have similar facts of glaciation in two countries so far removed, unless they were due to general and not to local causes. The advance of an ice-sheet from the south-east accounts for the phenomena; the drainage of the eastern coasts would be obstructed, whilst local glaciers would form, or be greatly increased where they now exist, by the precipitation of frozen moisture from the south-west winds when the mountains were raised by the lowering of the ocean-level, 2000 feet higher above the sea than they now stand.

Excepting the formation of the plains, I do not know of any evidence in South America of the advance of the ice upon the eastern coast; but in New Zealand Capt. Hutton has described an immense accumulation of clay containing angular blocks of mica-schist, many of large size, on the eastern or coast side of the Taieri Plain, and extending from the Taieri River nearly to Otakaia, a distance of about 3 miles. This deposit is many miles to the eastward of any known extension of the inland glaciers, and Capt. Hutton ascribes its origin to an earlier glacier period than the last. He says that there is a similar mica-schist on the sea-coast at Brighton, but rejects the supposition that they could have come from thence, because "many of the blocks are considerably above the highest level of the mica-schists there, and there is no conceivable agency by which they could have been brought from there."* The formation of this morainic deposit, and the transport of blocks of mica-schist inland, so difficult to account for by the extension of local glaciers, is just what we might expect on the theory of the advance of the ice from the south-east. No broken shells have been found in this deposit, such as mark, in other parts of the world, the drift left by glaciers that had crossed ocean-beds; but perhaps further examination may detect them.

It may be said, in regard to the formation of great lakes of fresh water by the advance of such enormous masses of ice upon the coasts, that the cold would be sufficient to freeze the lakes themselves and change them to masses of solid ice. But a minute's consideration will remove this objection. The temperature of the ice when it reached the coasts of New Zealand and South America would be little, if anything, below the freezing-point, and it

* Geology and Gold-Fields of Otago, p. 62.

would be melting itself instead of freezing the water it came in contact with. There is no more reason for the lakes it caused being frozen than that the Manjalen Sea—which is formed in a similar way—should be so.

The advance of the circumpolar ice-sheets in the form of low ridges, rising very gradually from their outer edges and culminating at a height of perhaps not more than 8000 or 10,000 feet above the present level of the sea, and then decreasing in height towards the Poles again, in consequence of the moisture being precipitated on the outer slopes, is, I think, more in accordance with our experience than the usual form of the theory of ice-caps, which would make them highest at the Poles. The latter supposition, it has been shown, is opposed to the facts that the ice flowed northward from the northern end of Scandinavia; that the high peaks of the Lofoden Isles are not glaciated; and especially that at the present time the northern end of Greenland is much more free from ice than the southern extremity. Whilst every inlet of South Greenland is occupied by ice flowing from the interior, and breaking off into great bergs when it arrives at sufficiently deep water, in the extreme north the glaciers do not reach the level of the sea. And in the Glacial period, instead of there being more ice than now within the Arctic and Antarctic circles, there was probably much less. Within the ridge of ice that then, I think, irregularly encircled the southern hemisphere, and, in the northern, bridged across the northern ends of the Pacific and Atlantic Oceans, both plants and animals that could hybernate through the severe and long winters might have existed in greater abundance than now. Even now, in the Antarctic, behind the ridge of ice that surrounds the South Polar continent, it is possible that there may be large areas of land free from ice in summer, and supporting a flora and fauna, which, if they could be studied, might throw much light on the distribution of animal and vegetable life in the South Temperate zone. And were that barrier of ice once passed a journey to the South Pole might be found to be less impracticable than one to the northern extremity of the globe.

Whilst the physical phenomena of the Glacial period in New Zealand and South America are so similar, there are differences in its effect on the pre-glacial fauna that must be noticed. Although there was great destruction of life amongst the individuals of the great apterous birds of New Zealand, there was not the same extirpation of species as

in America, nor even so much as in Europe amongst the great mammals. Numerous bones of the moas are found in the stratified deposits of the plains, but the same species reappear in the post-glacial surface-beds. Their complete destruction did not take place at the same time as the megatherium and its associates in America, or the mammoth and the woolly rhinoceros in Europe, but must rather be correlated with that of the Irish elk and the American mastodon. The preservation of the large birds through the great vicissitudes of the Glacial period may have been owing to the following causes:—Long before the ice from the Antarctic reached the coasts of New Zealand, a great stretch of land to the north would be laid dry by the lowering of the sea-level, so that there was possibly more space suitable for their occupation during the greatest extension of the ice than now. The same lowering of the sea-level took place all over the world, and in South America a similar extension of land surface must have ensued; but there the new land, to the north of the districts glaciated or submerged beneath the waters of the great glacial lake, would be occupied by northern animals who would resist the immigration of those fleeing the catastrophes of the south. In New Zealand, also, there was not the same competition with smaller animals, which appears to have led to the extirpation of many of the bulky species of the continents during the changing conditions of the Glacial period. Had, for instance, New Zealand at that time become connected with Australia, the great extension of area thus obtained, instead of tending to the preservation of the moas, would have probably led to their extermination by bringing them into competition with the marsupials of the continent. But a channel, 2600 fathoms deep, separates the two countries; and that there was then no land connection between them is evidenced by the absence of the gum-trees and acacias of Australia from the flora of New Zealand, and of the marsupials, the cockatoos, the grass parroquets, and the pigeons of the former from its fauna.

Although the large apterous birds of New Zealand were not exterminated during the great glaciation of the country, there are other signs of the impoverishment of its fauna. About 200 species of beetles had been described in 1872, and these belonged to no less than 110 genera, giving an average of less than two species to each genus. Many of the species live also in Australia, or have nearly allied forms there. The great paucity of insects, the isolation of the species, and their affinity with those of Tasmania and

Australia, is probably due to a great destruction of the native species during the Glacial period, and the arrival since of several from the countries to the north-west. I do not know how else some of the facts can be explained, such as that of there being only eight butterflies, and amongst these some of wide distribution; and that of the Heteroptera there are thirteen known species belonging to thirteen different genera and nine distinct families.* Such gaps as these in the fauna of a country are as significant as the grooved and polished surfaces of its rocks, and the naturalist may as surely point to the evidences of the Glacial period as the geologist.

Mr. Wallace has also drawn attention to the large destruction of species of insects in the Chilian sub-region, evidenced by the great number of peculiar genera of beetles of extremely isolated forms, and I might multiply instances from the faunas and floras of southern lands, all tending to the conclusion that the southern hemisphere has been glaciated as much as, or more than, the northern; but I could not do justice to this phase of the question within the limits of this article, and I have only glanced at some of its most salient points with the object of indicating that the physical evidence of glaciation does not stand alone, but is strengthened by that of the present distribution of animal and vegetable life.

* Capt. Hutton, Trans. New Zeal. Inst., vol. v., p. 227.

IV. RECENT ADVANCES IN TELEGRAPHY.

By J. Munro, C.E.

OF recent years the most important development of practical telegraphy has been the duplex system of sending messages. The idea of transmitting more than one message along a single wire at once appears first to have been conceived by M. Zantedeschi, as early as 1829; but we may take it for certain that it was not until 1853, or about twenty-five years ago, that any serious attempts were made to carry it out in practice. From that time the attention of electricians, both in Europe and America, has been directed to this interesting problem, but, until the last ten years, with so little success that in the 1867 edition of Sabine's "History of the Electric Telegraph" we find the following sentence:—"Both these systems of telegraphing in opposite directions, and of telegraphing in the same direction more than one message at a time, must be looked upon as little more than 'feats of intellectual gymnastics'—very beautiful in their way, but quite useless in a practical point of view."

Experience, however, shows that it is unwise to repudiate an electric novelty; and since these words were written the duplex system of telegraphing in opposite directions has become the ordinary means of communication over thousands of miles of land-lines in England and America, and of submarine cables in Europe and the East; while the multiplex system of sending several messages in the same direction is rapidly being brought into practical service by means of the Meyer instrument in France and the telephones of Elisha Gray in America and La Cour in Denmark. Indeed, by a future combination of the duplex and multiplex systems, we may yet have a single wire transmitting as many as twenty or more distinct messages, ten either way.

In order to explain the general principle of duplex or *counter* transmission, it is necessary to be perfectly familiar with the ordinary method of simplex or simple transmission. An ordinary telegraphic circuit invariably consists of the battery or source of the electric current; the key or sending instrument, by which the circuit is opened or closed

and the current admitted into the line; next, the line or wire stretching from the Station A, where the message is sent off to the Station B, where it is received; and next, the receiving instrument at Station B, actuated by the current from the line, so as to give sensible signals. These parts of the circuit are all connected together, and at

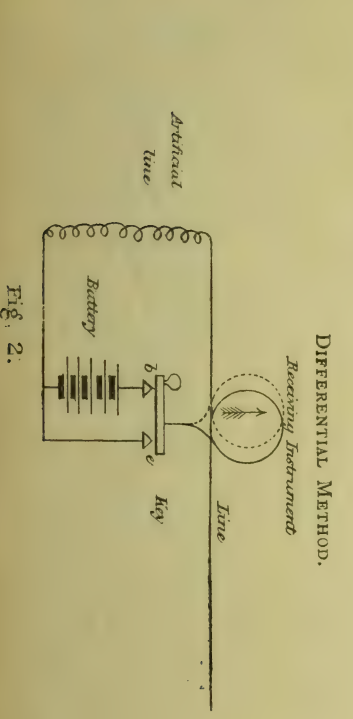


Fig. 2.

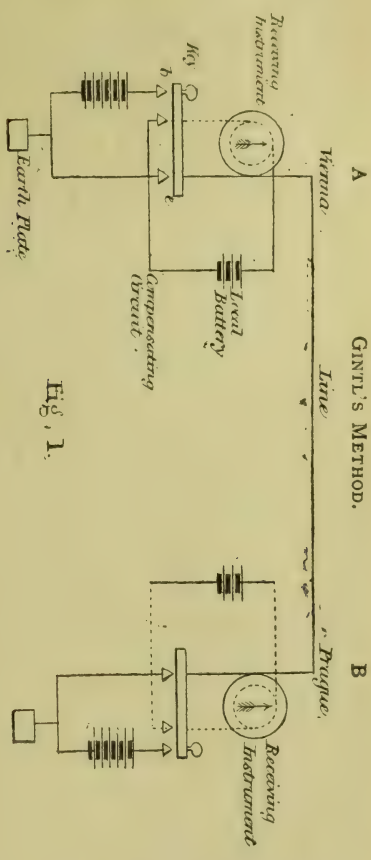


Fig. 1.

Station A one "pole" of the sending battery is put in connection with the ground by means of an "earth-plate," which is generally an unoxidisable metal, such as copper; while at Station B one "terminal" of the receiving instrument is connected, similarly, to an "earth-plate" there. A complete external circuit for the sending battery is thus formed through the line, the receiving instrument, and the

earth. This circuit is only interrupted when the lever or handle of the key is up. At such a time, of course, no current flows through the circuit. But when the sending clerk depresses the key the circuit is complete, a current traverses the line, and a signal is received at Station B. Combined according to the Morse code, such signals make up the message.

At first sight it seemed impossible that two messages could be sent, each in an opposite direction to the other, at the same time through the line. How was it possible that the messages could pass each other without interfering? If currents of equal strength were sent in opposite directions, would they not neutralise each other? In that case no current would traverse the wire at all, and what would become of the messages? Yet it is precisely on the ground that two currents of equal intensity do eliminate each other that duplex telegraphy is accomplished. The feat has been done, not by discovering or inventing a means whereby two currents could pass each other in a wire without disturbing one another, but by ingeniously joining up the sending and receiving apparatus at each end of the wire, so that the very interference of the currents, the one with the other, should cause the instruments to signal.

Such an arrangement was necessarily different from that used in ordinary transmission, as described. There was necessarily both a battery and a receiving instrument at each end; but the whole secret of the duplex system consists in the placing of the receiving instrument. All the methods hitherto invented agree in this—that the receiving instrument at either end is so placed that currents leaving the station where it is cannot cause it to signalise, whereas currents coming in from the distant station can. Again, should the currents leaving the station where it is be by any means stopped, this will have the effect of making it signalise. In describing the leading methods we shall show in each case how these conditions were obtained.

Dr. Gintl, a director of Austrian telegraphs, may be regarded as the founder of duplex telegraphy. Gintl, in 1853, described his system to the Academy of Sciences, Vienna, and practically tried it on the land-line between Vienna and Prague. Gintl's original system is shown in Fig. 1, which represents the arrangement of apparatus at Vienna and at Prague, with the line wire between. Here the receiving instrument is placed between the battery and the line, so that, when the circuit is completed, by depressing the key the current flows through the receiving instrument into the

line. With an ordinary instrument this current would produce a signal, but it is essential for duplex working that the sending from a station should not affect the receiving instrument at that station. Therefore, in order to counteract the effect of the signalling current on the instrument, Gintl employed a second smaller battery with its local or "compensating" circuit, and whenever the signalling current passed through the receiving instrument into the line he also caused a current from this second or compensating battery to pass in an opposite direction through the instrument, so as to neutralise or balance the effect of the signalling current. For this purpose the instrument was wound in opposite directions by two wires, one long and thin, represented by the full line, the other short and thick, represented by the dotted line. The long wire was in circuit with the sending battery and the line; the short wire was in circuit with the small battery and the compensating circuit. A peculiar key having double points was used for sending, so that when it was depressed it closed both the line and compensating circuits. The signalling current then rushed into the line by way of the long wire of the receiving instrument; but although it thus passed through it, it failed to make the receiving instrument signalise, because the current from the compensating battery passed simultaneously in an opposite direction through the short wire. These two currents were accurately adjusted to balance each other in their effects on the needle or indicator of the instrument. The essential condition of having the receiving instrument so placed as to be undisturbed by the sending was thus fulfilled, while at the same time it was in circuit with the line, and therefore free to receive signals from the distant station. Gintl's system had two serious defects:—In working the key in sending, the line was insulated for the moment when the lever was passing from the earth-contact, *e*, to the battery-contact, *b*. And the compensating battery was found to spend its strength quicker than the signalling battery. The harm arising from the first fault was that the received currents did not get freely to earth, and were therefore "broken up." The non-equivalence of the batteries, of course, destroyed the balance of currents on the receiving instrument, and "false" signals due to the sending were the result.

Gintl's plan, however, served as a stepping-stone to something better. Herr Carl Frischen, a telegraph engineer of Hanover, in the year 1854, greatly improved it by making the receiving instrument *differential*,—that is to say, instead of winding it with two dissimilar wires, as Gintl had done,

he wound it with two wires as similar as possible, but in opposite directions. It is unnecessary, then, to employ two different batteries. It is sufficient to split up the current from the signalling battery, sending one-half through one wire of the instrument into the line, and the other half through the other wire into the compensating circuit, which in this case must be made exactly equivalent to the line. This method of Frischen's has been called the Differential Method. It was re-invented a few months later by Messrs. Siemens and Halske, of Berlin, and is therefore sometimes called the Frischen-Siemens method.

Fig. 2 shows the arrangement of apparatus at either station. The receiving instrument is wound in opposite directions by two similar wires. The two adjacent ends of these two wires are joined to each other and to the lever of the key. The free ends are connected, one to the line, the other to the compensating circuit which—since it must be equivalent to the actual line—may now be called the

WATER ANALOGY.

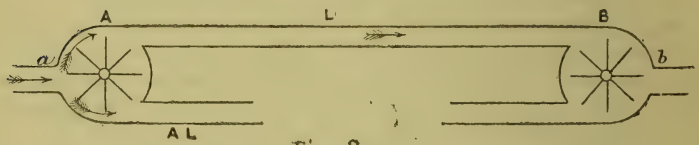


Fig 3

"artificial line." It will be seen that on depressing the lever of the key to the battery-contact the current splits at the point where the wires of the instrument branch off, and, when the artificial line is electrically equal to the actual line, exactly one-half will flow through the wire (drawn full) into the actual line, while the other half will flow through the wire (drawn dotted) into the artificial line. These two halves will counteract each other's effect on the needle, and the required electric balance will be obtained. Another advantage of this method over Gintl's was that the line-circuit is never absolutely interrupted while keying goes on; but it has this defect—that when the lever of the key rests on the earth-contact *e* the line is direct "to earth," whereas when it passes from *e* to *b* the line is to earth through the artificial line. Such a variance in the earth-contacts is attended by variance in the size of the received signals.

A convenient means of elucidating the duplex principle, in the case of the differential method, is furnished us by a

water analogy. Let us suppose that Fig. 4 is a peculiarly-shaped water-pipe. The spoked wheels or vanes in the enlarged parts of the pipe are free to rotate, and represent the needles or indicators of the receiving instruments at the Stations A and B. The long connecting-pipe, L, represents the line between these stations, and the short pipes, A L, stand for the artificial lines. Confining our attention to Station A, if we suppose that water at a certain pressure is admitted into the pipe at *a*, it will divide itself into two streams, one of which will flow over the vane into the pipe L, and the other will flow under the vane into the pipe A L, as shown by the curved arrows. If the pipes are so constructed that these two streams are equal in force the vane will remain stationary, for they will balance each other's

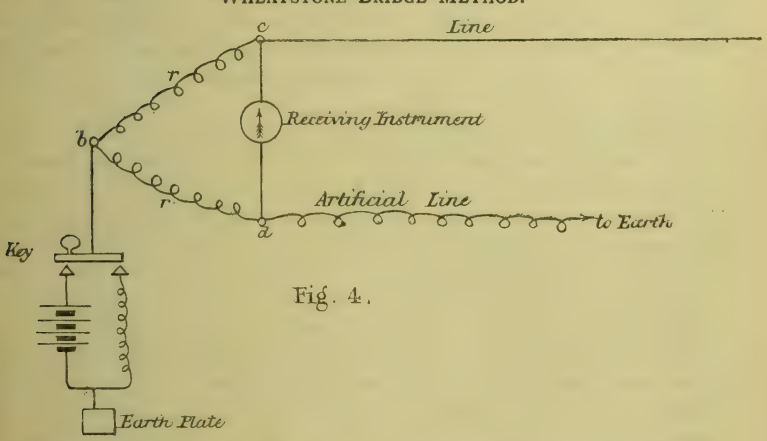


Fig. 4.

effect on it. But if by some means the stream flowing along the pipe L should be stopped, the balance on the vane would be disturbed, and it would be seen to rotate. Such an effect would be produced by admitting a like stream of water into the pipe by the orifice *b* at the other end. When the water is simultaneously admitted into the pipe at both ends the two opposing streams in the pipe L stop each other, and simultaneously both vanes move. Thus, when both stations admit water, both vanes move. We thus arrive at the two elementary cases of duplex working. The first case is when one station (say A) is sending a signal, while the other station is not. We have seen that in this case the water is only admitted at A, and the vane there is motionless; but the stream which flows along the pipe L rotates

the vane B: thus a signal is made there. This corresponds to Station A signalling Station B. The other case is when both stations are sending to each other. In this case the water is simultaneously omitted at both ends of the pipe, and both vanes rotate. This corresponds to both stations signalling each other. Out of the distinct signals the message is of course made up. In duplex sending these two elementary cases are constantly recurring, owing to the pauses between letters and words. We have, for the sake of simplicity, considered that the sending current used at each station is of the same strength and of one kind, either positive or negative. But in submarine telegraphy both positive and negative currents are used in sending, so that a third case has to be added to the above—namely, when Station A is sending a positive and Station B a negative signal. A little reflection will, however, show that this is merely a variety of the second case. It will be seen, too, that the principle still holds good when the batteries used at the two stations are of unequal strength; for the effect will only change in degree.

Immediately after the invention of the differential method there was a great deal of activity displayed in duplex experiments, both at home and abroad, especially in Germany and America. The system was even in practical operation on many of the Prussian land-lines, but it gradually fell into disuse, chiefly through the difficulty in maintaining the equivalence of the real and artificial lines, owing to imperfect insulation of the wires. Some desultory progress was still exhibited here and there, however, showing that the idea was only in abeyance until circumstances were more favourable to its development. The most noticeable of these improvements was the plan of putting the receiving instrument in the diagonal wire of a Wheatstone balance formed of the actual line, the artificial line, and two other proportional resistances. This has been called the "Wheatstone Balance Method." It originated with M. Maron, of Berlin, in 1863. The arrangement of apparatus at each station is shown in Fig. 4. The proportional resistances, $r\,r'$, are usually made equal to each other, so that by the principle of the Wheatstone balances the artificial line must be made equal to the actual line. When this is so there will be no effect on the receiving instrument placed in the diagonal $c\,d$ when the sending key is depressed and the current from the battery admitted into the system. The current will divide itself at the point b, and one-half will flow by the branch r into the real line, and so on to the distant

station, while the other half will flow by the branch *r'* into the artificial line, and so to earth. These currents being of equal *potential* as they pass the points *c* and *d*, no cross current between them will flow through the diagonal and the receiving instrument. A balance on the receiving instrument will be obtained, and the sending at the home station will not affect the receiving instrument there. The sending at the distant station, however, will still have control over the home instrument, for by the stoppage of the line-half of the home-sending current the balance will be disturbed, and the home-receiving instrument will signalise.

We have already seen, by help of the water analogy, in the case of the differential method, how that when only one station is sending a current only the receiving instrument at the station it is sent to receives it; and that when both stations simultaneously send currents, both instruments signalise. The water analogy can also be readily applied to elucidate the same results in this method by supposing the real line and the artificial line to represent two equal streams of water flowing down equal channels. The conditions are so arranged that there is the same volume and head of water (potential) at the points *c* and *d*, and the same rate of fall of the level of the channels. In such a case, were a cross channel to be made between *c* and *d*, and a mill-wheel placed in it, there would be no cross current in it to turn the mill. But if by some means, such as shutting a sluice-gate, the stream in the line were suddenly dammed back, then the level of *c* would rise—a cross current would set in towards *d*, turning the wheel on its way. The act of sending at the distant station is comparable with the closing of the sluice.

The next important advances in duplex telegraphy were made in 1868, by Mr. Joseph Barker Stearns, an American electrician, who has done much to place it on its present footing. Stearns's principal improvements were—the use of a peculiar key, which permitted the line always to be "to earth" through a constant resistance, and the addition of a condenser or other inductive apparatus to the artificial line, so as to imitate the sensible induction of the earth on long land-lines. Stearns applied these improvements to both the differential and Wheatstone balance methods. Fig. 5 shows their application to the former. He found that on land-lines of over 400 miles in length the sudden static charge due to the earth's induction caused a sudden "kick" or false signal on the receiving instrument; and in order to counteract it and secure the perfect balance required, he

gave the artificial line an equivalent inductive capacity, and produced a counterfeit "kick" in the opposite direction, by the addition of the condenser. Stearn's system has achieved a wide application in the United States.

STEARNS'S KEY.

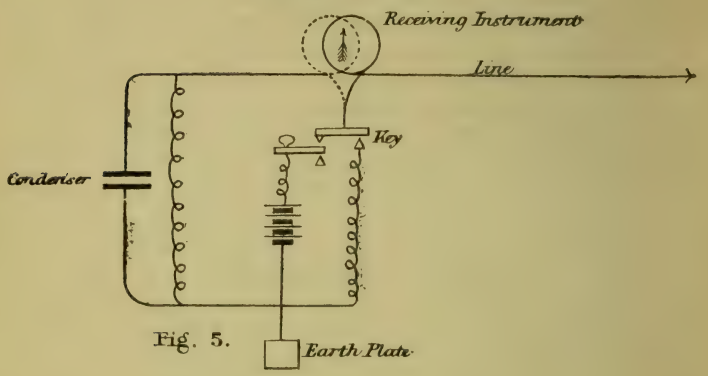


Fig. 5.

Another successful duplex method on land-lines is that of Mr. G. K. Winter, of Madras. It is called the method of "opposed batteries," and consists essentially of the "differential method;" but whereas in the latter the battery circuit is only completed in the act of sending a signal, in the former the circuit is closed, so that the batteries at each station oppose each other through the line.

Stimulated by their successes on land-lines, both Winter and Stearns, as well as others, in 1873 began to turn their attention to the far more difficult task of duplexing submarine cables. The marked inductive effect of the earth on a submarine cable—which resembles an attenuated Leyden jar—greatly complicated the requirements of the balance. The electric signal travels through a cable in the form of a wave, and is sensibly retarded by the earth's induction.

How to produce an artificial line which should be electrically equivalent to the actual cable in all its effects on the current. This was one way of solving the problem. Another lay in finding some contrivance or device (mechanical or otherwise) which should counteract the effect of the prolonged inductive charge of the cable on the receiving instrument, so as still to produce a balance.

As early as 1862 Mr. Varley had invented an artificial cable, for use as a circuit on which to test the working of

telegraphic receiving instruments. It consisted of a combination of resistance-coils and condensers—the resistance-coils, of course, imitating the conductor of the cable, and the condensers its inductive capacity. Such an artificial cable is shown in Fig. 6. The condensers, or accumulators

VARLEY'S ARTIFICIAL LINE.

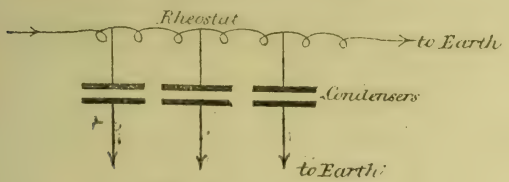


Fig 6.

as they are sometimes called, are regularly distributed along the rheostat. The current is made to pass through the rheostat or resistance-coils to earth, as through the conductor of a cable, and in its passages it charges the condensers, which in this come to imitate the induction of the earth.

Stearns made use of Varley's artificial cable in his experiments in submarine duplex. He modified it to a slight extent by attaching all the condensers together at one point of the rheostat, and inserting resistances between them, so as to regulate the charges which they took up. For instance, as shown in Fig. 7, he would insert an adjustable resistance-coil between the rheostat and a condenser, and another coil between two condensers themselves. He had thus a means

STEARNS'S ARTIFICIAL LINE.

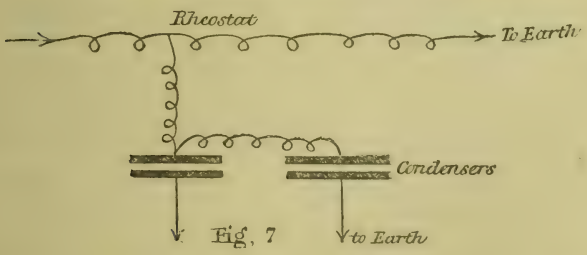


Fig. 7

of controlling the charges of the condensers so as to bring the artificial line, if possible, into harmony with the cable. But experience has shown that such a combination of condensers and resistance-coils does not approximate to a cable sufficiently for the purposes of practical duplex working on

long submarine cables. Nevertheless, by the help of the *devices* above mentioned, it is probable that cables of a considerable length might be *duplexed* with an artificial line of this kind. As yet, however, this has not been accomplished, except experimentally.

What is now known as Muirhead's system is really the first successful method of working submarine cables on the duplex principle. Mr. John Muirhead, jun., first patented this system in 1874, and since then it has been rapidly developed and applied, chiefly by Dr. Muirhead and Mr. Herbert Taylor, C.E. The system includes both a novel artificial line, or model cable, and various adjustments or devices for bringing the artificial line into close equivalence with the cable, and for perfecting the balance. Muirhead's artificial line aims at complete equality with the cable, as far as it can practically be done. Like a real cable, it is an intimate union of resistance and capacity, and is called an "inductive resistance." It is formed "by taking two strips of tin-foil, and laying one over the other separated by an insulator, such as paraffined paper. One strip forms the conducting circuit of the artificial line; the other forms the outer or inductive coating, and is connected to earth. The current is passed through the conducting strip, and exposed

MUIRHEAD'S ARTIFICIAL LINE.

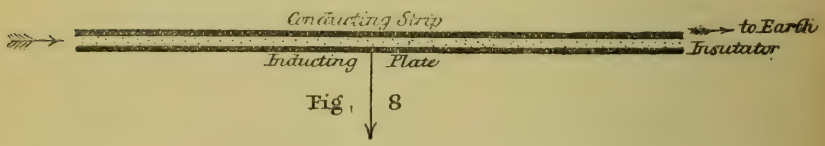


Fig. 8

throughout its entire length to the induction of the other strip or sheet in the same way that the current in the conductor of a cable is subjected to the induction of the earth "throughout its entire length." This is shown in Fig. 8, where the arrows represent the local current passing along the conducting strip, while being at the same time retarded by the induction of the other strip, or inducting-plate, which is connected to earth. This model cable can be made to have the same resistance, capacity, and even leakage, per knot that the actual cable has, so that it may be practically equivalent to the actual cable. For very long lines, however, and with such delicate receiving instruments as Sir William Thomson's "mirror galvanometer" and "siphon recorder," it is in general necessary to refine upon the balance by means of extraneous adjustments. It may be

said, however, that the more nearly the model cable is made to equal the real one, the less needful is any other help. The use of such additional devices is, as we have said, to counteract the "kick," or false signal, on the receiving instrument, due to a want of perfect accord in the balance between the real and artificial lines. Thus it will be seen by a reference to Fig. 4 that if there is any difference between the rush of electricity into the cable at *c* and the rush into the artificial line at *d*, a momentary pulse will traverse the receiving instrument and jerk the needle; this will take place at every signal sent from the station. The needle thus "kicked" by the home sending is of course unable to deliver the message from the distant station. It is comparatively easy to get rid of the violence of the kick and to reduce it to a slight jar or tremor, but it is a difficult matter to eliminate it altogether. This, however, has been practically achieved by Muirhead's system.

The principal device for obviating the "kick" is to counteract its effect on the instrument by superposing an equal "kick" in the opposite direction. This can be done by allowing a battery-current to flow for an instant through an extra coil of the receiving instrument. But it is generally effected either—

By connecting a condenser to the instrument, so that a sudden inductive charge into it shall produce the kick required; or—

By using a "secondary battery" (made of a series of plates of lead or of platinum in a weak solution of sulphuric acid) in the same way as the condenser; or—

By employing an induction-coil and placing the primary in circuit, somewhere, with the signalling-current, while the secondary is connected to the receiving instrument; so that the sudden induced currents generated in it by each signal-current may produce a "counterfeit kick" in the opposite direction to the real one, and so completely neutralise it.

But although these plans of neutralising the kick by an equal and opposite one are doubtless serviceable, they introduce complications into the balance which are to be avoided, if possible, in practice. By the use of the inductive-resistances, and a simple but effectual adjustment which is applied to them, the kick is practically reduced to *nil*, and a perfect balance obtained, so that constant sending in opposite directions can go on simultaneously with signals as beautiful as in the ordinary method of working.

Muirhead's system was first experimentally tried, by Dr. Muirhead and the writer, on the Marseilles to Bona (Algeria) cable of the Eastern Telegraph Company, in 1875. The following year it was established for the regular traffic on the Marseilles to Malta section, and on the Suez to Aden or Red Sea cable, which is 1460 miles long, and in an electrical sense one of the longest of existing cables. Early in this year it was established on the Aden to Bombay section, a cable of 1817 miles long. There are thus upwards of 4000 miles of this Company's lines to India worked on the duplex system, and it has been given out that the Company hopes this year to apply it to all their cables.

The duplex system of transmission is without doubt a great practical gain, since it doubles the carrying capacity of a line, and makes it speak, as it were, with two tongues. With such facts as the above before us we can no longer regard it as a "species of intellectual gymnastics." It has been brought at last to a serviceable issue, and bids fair to become a considerable public benefit.

Quadruplex Telegraphy.

Very soon after Dr. Gintl, by his invention of the duplex system, had shown the feasibility of simultaneously transmitting two messages counter to each other along one wire, electricians engaged their minds with the allied problem of transmitting two messages simultaneously in the same direction. And they were not backward in foreseeing that by the union of both methods a system of quadruplex telegraphy would be placed in their hands. On the 27th of October, 1855, Dr. J. Bosscha, jun., of Leyden, at a meeting of the Royal Academy of Sciences, Holland, described a method he had invented for the simultaneous sending of two messages in one direction along one wire, and likewise pointed out that, by the union with it of the Frischen-Siemens duplex system, a means of sending four messages simultaneously was provided. On the 31st of the same month Dr. J. B. Stark, of Vienna, published his method of double-sending in the same direction, and concluded his account as follows:—"With the method of double transmission in the same direction we may also combine that of counter-transmission (*gegensprechen*), and hence arises the possibility of simultaneously exchanging four messages upon one wire between two stations, which will, however, hardly find any application in practice."

Following Stark and Bosscha, other electricians in Europe during the following year invented methods for effecting the same purpose—notably Drs. Werner Siemens, Kramer, and Bernstein, of Berlin. All these early methods agree in principle, although they differ in detail. The problem of double transmission implies the use of two keys or sending instruments at one end of the line, and of two or more receiving instruments at the other. The use of two keys simultaneously involves four combinations of their action, namely:—

1. When No. 1 is closed and No. 2 is open.
2. When No. 2 is closed and No. 1 is open.
3. When both are closed.
4. When both are open.

These four conditions of the key should give rise to four distinct electrical conditions of the line, which are capable of being interpreted by the receiving relays. In the method of Bosscha, Kramer, and others, these electrical conditions were, for—

1. A positive current of strength 1 sent.
2. A negative current of strength 1 sent.
3. A positive or negative current of strength 2 sent.
4. No current sent.

They were interpreted at the receiving end of the line by one polarised and two neutral relays. In the method of Stark, Siemens, and others, the electrical conditions were—

1. A positive current of strength 1 sent.
2. A positive current of strength 2 sent.
3. A positive current of strength 3 sent.
4. No current sent.

These were interpreted at the receiving end of the line by three neutral or unpolarised relays of different degrees of sensitiveness.

The defects of these early systems lay in part with the keys and in part with the receiving relays. When the keys were being worked there were momentary interruptions of the line-circuit in changing from one contact to another. It was found to be a difficult matter to keep the three relays at their proper degree of sensitiveness in Stark's system. In Bosscha's system the sudden reversal of magnetic polarity in the neutral relay, caused by the reversal of currents, had the effect of breaking up the signals of the neutral relay. These difficulties, however, might all have been overcome had it not been for the additional disturbances or "kicks" due to the static induction of the line. These

QUADRUPLEX TELEGRAPHY.

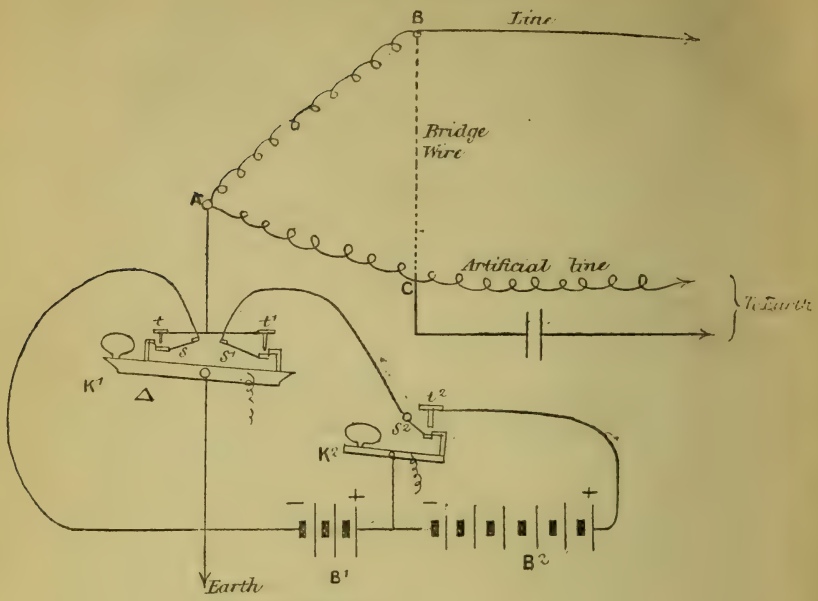


Fig. 9.

QUADRUPLEX TELEGRAPHY.

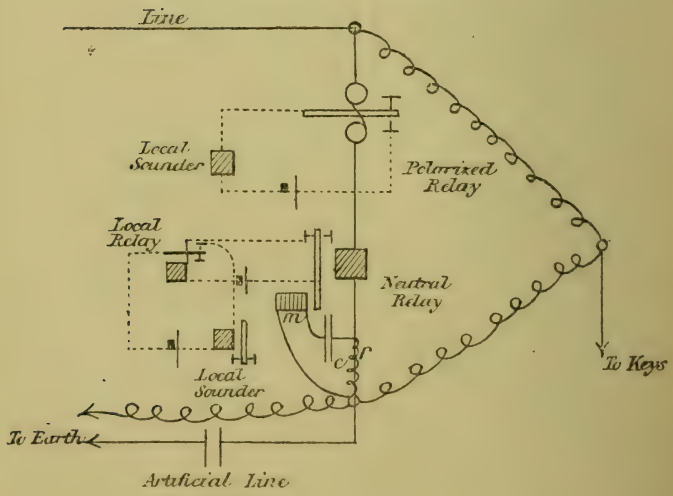


Fig. 10

proved fatal to the early duplex systems, and although there was up till 1865 considerable ingenuity shown, nothing practical was done in double transmission.

The recent revival of duplex, however, again revived the hope of double transmission and of quadruplex. In 1872 Mr. Stearns made duplex a practical success by applying the condenser to the artificial line, and thereby overcoming the "kicks;" and in 1874 Mr. T. Alva Edison, of Newark, New Jersey, and Mr. G. B. Prescott, while testing Stearns's system, invented the first practical system of quadruplex. It was worked, in the same year, on the New York to Boston line, a distance of 240 miles. Since then it has been introduced on several long lines in the United States, including the line from New York to Chicago, which is nearly 1000 miles in length. On this line there is a "repeater" at Buffalo. The speed of working is about 120 words a minute.

The basis of Messrs. Prescott and Edison's quadruplex system is the "Wheatstone bridge" duplex system. With this is united their system of double transmission, which resembles the early method of Bosscha, but differs from it in many important points. To understand their system it is only necessary to consider their plan of double transmission, since we know that the mere duplexing of this arrangement ensures quadruplex. The combination of currents produced by the two keys are for—

1. A positive current of strength 1 sent.
2. A negative current of strength 3 or 4 sent.
3. A positive current of strength 3 or 4 sent.
4. A negative current of strength 1 sent.

There is a practical advantage in this system over the earlier ones, in the greater disparity between the different currents. To interpret these currents a Siemens polarised relay and a neutral relay are used. The neutral relay will work by currents, either positive or negative, provided they are sufficiently strong. The currents sent by 2 and 3 work this relay. The polarised relay depends for its working only on the *kind* of the current, not on its strength. Thus a negative current keeps the armature or tongue of the relay against its back stop, thereby keeping the local circuit open, whereas a positive current causes the tongue to close the local circuit. This relay therefore works with 1 and 3 currents.

Fig. 9 represents the arrangement of the keys and batteries for sending. A B C is the "Wheatstone bridge," with

the bridge wire B C, in which the receiving relays are placed. K_1 and K_2 are the signalling-keys. B_1 and B_2 are the batteries. K_2 is an ordinary "single-current" key, whose function is simply to add or take out the battery power, B_2. K_1 is a "double-current" key. When in its position of rest, as shown, a negative current of strength 1 continually flows to line from the − pole of B_1, through the spring *s*, and *t*. This negative current is too weak to close the neutral receiving relay, and it is of the wrong kind to close the polarised relay. When, however,—

Case 1, this key is closed, the positive current from B_1 flows to line through the spring s_1 and t_1. It is too weak to close the neutral relay, but it closes the polarised relay.

In Case 2, when K_2 is closed and K_1 is open, the + pole of B_2 is to earth through t_2 and springs s_2 and s_1, while the − pole of B_1 is to line through *s* and *t*. Thus both batteries act conjointly, and a negative current of triple strength flows through the line. This has the effect of closing the neutral relay and leaving the polarised relay open.

In Case 3, similarly, it will be found that a positive current of triple strength enters the line. This closes both relays.

In Case 4, when both keys are open, as shown, the − pole of B_1 is to line, and a negative current of strength 1 flows. It has the effect of keeping both relays open.

Fig. 10 represents the arrangement of receiving relays with their local circuits. R_1 is the polarised and R_2 the neutral relay. The local circuit of R_1 is made up of a local "sounder" and actuating battery; that of R_2 is made up of a local relay battery and "sounder." The addition of this local relay is to overcome the defect experienced in the early systems of the breaking-up of signals in the neutral relay due to sudden reversals of current in the line. A little consideration will show that the current may be reversed in the line by the key K_1 while a signal is being made on K_2. This reversal of polarity in the relay involves an intermediate moment of no polarity, during which the tongue t_2 will fall away from its contact. This causes a break in the signal made by the local sounder. The intervention of a local relay, however, was found to delay the action of the sounder, so that this interval of falling off could be bridged over, so to speak. On long lines the static induction, by delaying the change of polarity in the relay, extends this interval so

much that the local relay no longer avails, and recourse must be had to another device. This consists in adding a derived or loop circuit to the bridge wire, consisting of a condenser, *c*, electro-magnet, *m*, and adjustive resistance, *r*. The electro-magnet is set so as to act upon the other end of the tongue of the neutral relay, as shown. In this way, when a change of current takes place, the discharge from the condenser through *m* serves to keep the tongue in its place until the critical time is past; *r* is merely for adjustment. By these ingenious contrivances Messrs. Prescott and Edison have rendered their quadruplex system a practical success. Within the last few years other systems, more or less similar, have been devised, but as yet theirs is the only one of note.

Telephonic Telegraphy.

a. *The Tone Telephone.*

The last five years have seen the practical development of this interesting branch of telegraphy. For many years back attempts have been made to transmit musical tones or articulate sounds to a distance by means of electricity, but the results were at best only hopeful until, in 1860, Herr Phillip Reis, of Homburg, profiting by the researches of Wertheim, Marian, and Henry, invented the first telephone. In Reis's telephone a stretched membrane is set into vibration in unison with the sound to be transmitted, and, by a little contact-piece which it carries, this vibrating membrane is caused to interrupt the electric current circulating in the line. The current so interrupted is utilised at the distant station to produce a sound similar to the original sound at the near or sending station. To produce such a sound Reis made use of Page's discovery, that an audible "click" accompanies the demagnetisation of a bar of iron inserted in an electro-magnetic helix. He surrounded an iron wire with a helix, and caused the interrupted current from the sending station to pass through the helix to earth. At every interruption a distinct sound was given out by the iron core, and the joint effect of these reproduced a note of the same pitch as that sounded at the sending station.

In telegraphy, however, the telephone only acquires special importance when it is regarded as a means of multiplex transmission. A single message by telephone can have small advantage over the ordinary methods; so Reis's telephone was neglected as a kind of scientific curiosity.

Ten years later, in 1870, Mr. Cromwell Fleetwood Varley, F.R.S., appears to have first clearly designed the employment of the telephone in the transmission of several messages simultaneously on one line wire, by means of separate notes. Varley's patent of that year is full of most ingenious plans and contrivances, not only for the sending and receiving of telephone messages, but for rendering the vibratory electric signals visible, both temporarily, by light, and permanently, by recorded marks on moving paper. The principles of later inventions are to be found here, and even some of the details. But Varley seems to have left the carrying out of his system in abeyance; and the merit of practical success belongs to the systems of M. Paul la Cour, of Copenhagen, and Mr. Elisha Gray, of Chicago.

The principle of all these systems is that a vibrating body, such as a tuning-fork or membrane, emitting a certain note, shall be caused at each vibration to interrupt an electric

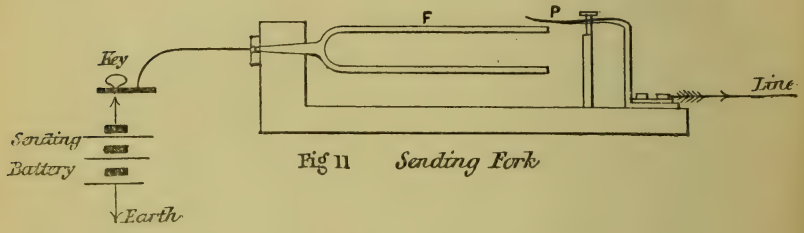


Fig 11 *Sending Fork*

current in the telegraph line; and the current so interrupted shall, at the distant station, be made to set a corresponding body in vibration, so as to reproduce the original note there. Of course it is possible, by having a number of vibrators at the sending station and a corresponding number of vibrators in unison with them at the receiving station, to give rise simultaneously to several distinct sets of electric vibrations in the line wire, and to reproduce several distinct notes at the receiving station. In this way is multiplex transmission rendered feasible.

M. Paul la Cour's English patents bear date 1874 and 1876. Briefly, his system is as follows:—Fig. 11 represents his sending arrangement. F is a tuning-fork which is put into vibration, and makes contact once every vibration with the contact-point P. The fork is connected, through the signalling-key, with the sending-battery, and the contact-point P is connected to the line. Here the fork is simply shown, and must be started by hand; but in his later patent

M. la Cour provides, by means of electro-magnets, that the fork shall be maintained constantly in a state of vibration. In either case the current sent into the line by depressing the signalling-key is interrupted by the fork an equal number of times per second that the fork vibrates. This intermittent current traverses the line to the distant station, where it is passed "to earth" through the receiving apparatus shown in Fig. 12. This consists of a fork similar to the first, and vibrating the same note. Each leg is surrounded by a helix of wire. Two other helices are placed upright, one on each side of the ends of the legs; they are fitted with iron cores, and adjustible poles, *n*, *s*, and are in fact electro-magnets. These four helices are joined up in series, so that the line-current passes through each in turn. It

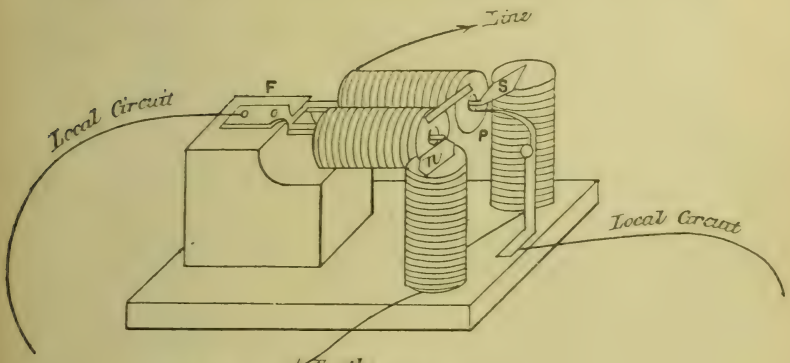


Fig 12. *Receiving Fork*

magnetises oppositely the legs of the fork, which are attracted by the contiguous poles of the electro-magnets. The legs of the fork are thus pulled apart, and let go with each pulsation of the intermittent current. In this way the fork is put into audible vibration.

These vibrations are also turned to the actual recording of the message in permanent marks, by means of a local circuit and Morse or other recorder. A fine metal point, P, is brought very near to one leg of the fork, so that when the fork vibrates this leg comes into contact with the point, and completes the local circuit through the fork, local battery, and recording instrument.

M. la Cour's first experiments were made in June, 1874, on a short line in the neighbourhood of Copenhagen, and in November of the same year he succeeded in working

successfully from Fredericia, in Jutland, to Copenhagen, a distance of 390 kilometres.

Mr. Elisha Gray's first English patent bears date of 1874, a few months before M. la Cour's; but in it there is no mention of multiplex telegraphy. Subsequent patents, in 1875 and 1876, describe the development of his system and its application to multiplex signalling. His principal methods of sending are represented in Figs. 13 and 14. In Fig. 13 the vibrator is a tuning-fork or reed, R, placed between two electro-magnets, A and B, which can be adjusted by the screws D and G to or from the reed, so as to have greater or less power over it. The reed is barbed with

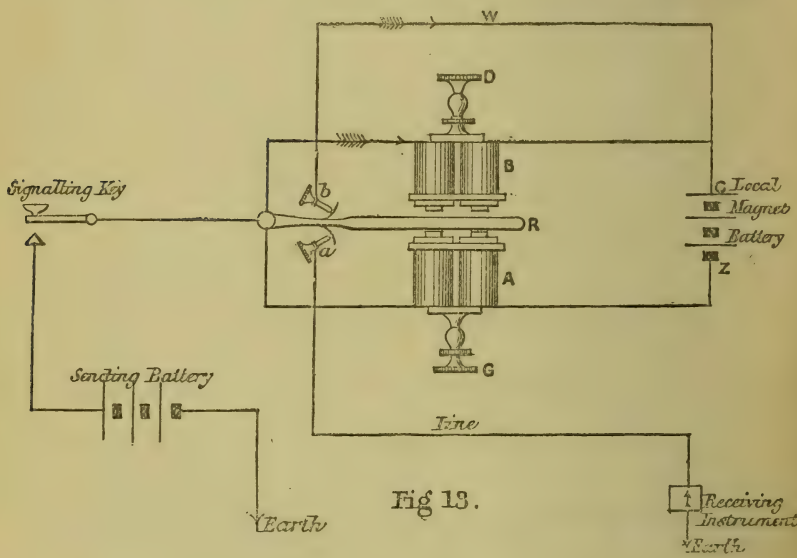


Fig 13.

two short springs, which play against the adjustible contact-points *a* and *b*, making contact with them alternately as the reed vibrates. The reed is perpetually maintained in vibration by means of a local battery and the electro-magnets in the following way:—The local battery is connected up, as shown, through the electro-magnets and the fixed end of the reed. When the reed vibrates so as to make contact, by means of its spring, with *b*, and break contact with *a*, the electro-magnet A is in circuit and actuated by the current, while the magnet B is cut out of circuit by the short-circuit wire W. The magnet A therefore exercises a pull upon the reed R, which assists the reed in vibrating towards

it, and breaking contact with *b* while making contact with *a*. This, while keeping the electro-magnet A still in circuit, also puts the magnet B in circuit. In this way the electro-magnet B is alternately thrown in and out of circuit, and

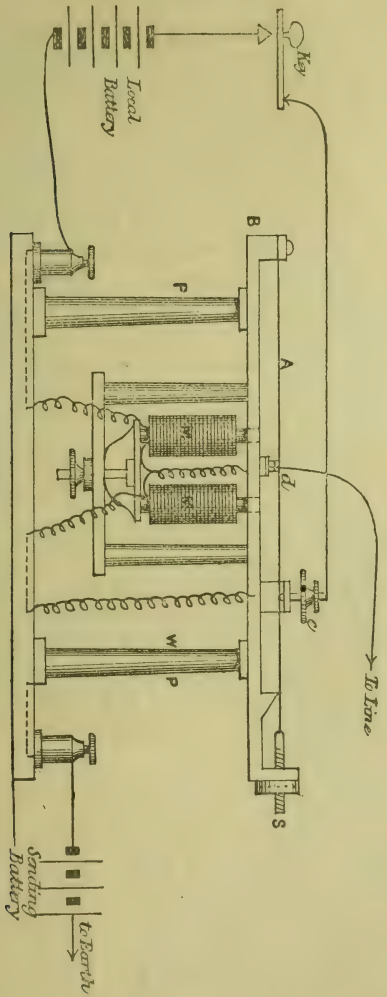


Fig. 14.

the effect of this on the reed is to keep up its vibration. The reed, thus kept vibrating by the electro-magnets and local battery, is used to interrupt the line circuit and sending current. The sending battery is connected up, as shown, through the signalling-key to the reed, and by the reed-

spring and contact-point *a* to the line. At each vibration of the reed the contact between the spring and *a* is made and broken, and the circuit opened and closed. Whenever the signalling-key is depressed, therefore, an intermittent current enters the line.

Mr. Gray also procures the intermittent current by means of a vibrating bar or string, as in Fig. 14, where A is a thin steel bar stretched by the screw S, and carried by a fixed metal bar or frame, B. The bar A vibrates between upper and lower contact-points, *c*, *d*. M M are two electro-magnets whose poles are let through the bar B, so as to act upon the vibrating bar. P P are pillars supporting the whole. In this arrangement a local battery is also employed to set and maintain A in vibration: this is done, however, in the act of signalling. On closing the signalling-key the local current passes to the upper contact *c*, against which the bar A rests. From thence it passes, by means of the bar B and wire W, through the magnets M M, completing its circuit. These magnets then attract the bar downwards in its middle, plucking it away from contact with *c*, and bringing it into contact with *d* underneath. The local circuit being thus broken, however, the bar springs back again into contact with *c*, to be plucked down again as before. In this way the bar is started vibrating. The line circuit is made through the lower contact *d*, the bars B and A, and the pillar P. At every vibration of the steel bar A it is interrupted, and an intermittent current set up in the line.

In both of these methods the number of intermissions in the line current will correspond with the number of vibrations of the vibrators. By employing in the first method reeds of different pitch, and in the last bars or strings of different dimensions and tension, distinct intermittent currents will be produced.

The principal receivers for interpreting these currents into distinct audible sounds are represented in Figs. 15 and 16. In Fig. 15 M is a double electro-magnet supported over a resonance pipe closed at one end. The soft-iron armature of the electro-magnet, *t* T, is rigidly fixed to one pole at *t*, the other end being free to vibrate in front of the other pole at T. When the intermittent current from the line passes through the electro-magnet the free end of the armature or tongue is set into corresponding vibration, and the air-column in the resonance box, vibrating in unison with it, gives out an audible note. It is easy to see how, by employing a number of separate transmitters, such as described, to superimpose separate intermissions in the line current,

and passing the complex current so produced through as many separate receivers of this kind, that each receiver will only respond to its own particular set of intermissions. For the sounding-pipe of each receiver can be so constructed

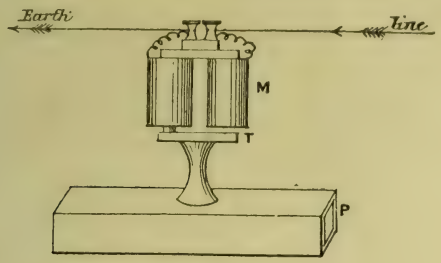


Fig 15.

as to resound only to the vibrations set up in the tongue by a particular series of intermissions. In this way several distinct notes may be simultaneously telegraphed, each note being used for a separate message.

The receiver shown in Fig. 16 has been called the "physiological receiver," since it depends for its action on the

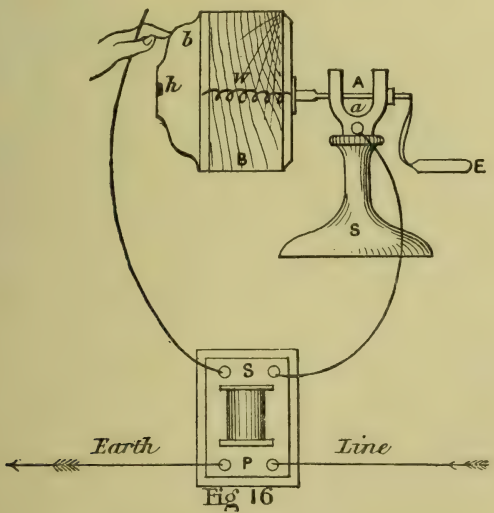


Fig 16

contact of living animal tissue with a conducting surface. It is the most interesting of the two, because its action has not hitherto been explained. If the intermittent current from the line is passed through the tissue to the conductor,

the corresponding note is faintly audible. To intensify the effect the line current, in practice, is simply passed to earth through the "primary," P, of an induction-coil, and the more intense secondary current is used. In the figure, B is a hollow wooden resonance-box, with a bulging zinc face, *b*. This box is carried by a metal axle, A, supported by a stand, S: it is rotated by turning the handle, E. The zinc face is connected by a wire, *w*, to the axle; *h* is merely an air-hole in the face of the box. One end of the "secondary coil," S, is connected by wire to the metal axle at the terminal *a*; the other end is connected to a bare wire held in the operator's hand, as shown. The operator lightly presses a finger of this hand on the zinc face, while with the other he rotates the box, and the dry rasp of the skin on the zinc surface is changed into a musical note whenever the current passes.

Like M. la Cour, Gray also provides that the vibratory current shall close a local circuit and record the message in permanent marks, by means of a Morse or other recording instrument. For this purpose he employs a receiver similar to the string-transmitter shown in Fig. 14. The line current, passing through the magnets of this instrument, sets the tense bar in vibration against the upper contact *c*, thus closing the local circuit and actuating the local recorder.

Mr. Gray's apparatus is now successfully operated over more than 2400 miles of the Western Union Telegraph Company's lines, including distances of several hundred miles. As many as four, and even eight, messages are simultaneously sent. Both he and M. la Cour are still engaged in perfecting their apparatus, and we may reasonably expect that the telephone will ere long do good service as a practical telegraph.

b. *The Articulating Telephone.*

This ingenious little instrument is the most wonderful of all the forms of telephone, and the latest, as it is the greatest, to use the words of Sir William Thomson, "of all the marvels of the electric telegraph." Its peculiar faculty lies in the transmission of promiscuous sounds. Not only does it convey the blended notes of musical instruments, but it actually reproduces the human speech. It is easy to see how an instrument like this will become practically useful. For domestic or commercial purposes, for reports of lectures and speeches it is especially fit. It has the advantage of quickness over ordinary methods of telegraphing.

In these, each letter of a word is on an average composed of three distinct signals; but in the articulating telephone a whole word is transmitted by the single act of uttering it.

Mr. A. Graham Bell, the inventor of this now-famous instrument, is, we believe, a native of Edinburgh, and is now a Professor of Boston University, and a naturalised

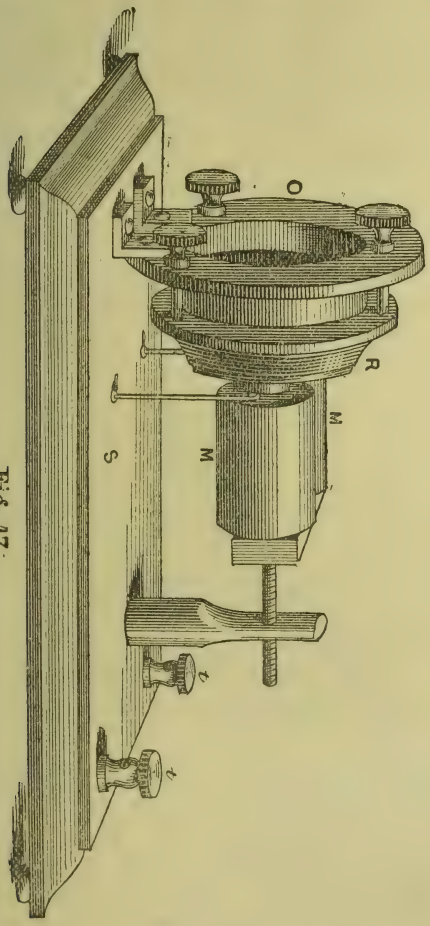


Fig. 17.

citizen of the United States. In December last he patented it in England.

The articulating telephone consists of two distinct apparatus, a sender and a receiver. Fig. 17 represents the sender, and Fig. 18 the receiver: they are both exceedingly simple. In the sender, M M are two coils of insulated wire

surrounding the two poles of a powerful permanent magnet. These coils are connected up together, and to the terminals *t t*, on the mahogany stand S. Immediately in front of the poles of a magnet is a membrane stretched on a ring, R. This membrane carries an oblong piece of soft iron cemented to it just opposite the poles of the magnet. A suitable acoustic cavity, or mouthpiece to speak into, O, fitted with three screws for tightening up the membrane, complete the apparatus. The sender speaks into the mouthpiece in an elevated voice, and the membrane, vibrating in unison, carries the piece of soft iron—which is really a movable armature—to and from the poles of the magnet. This has the effect of inducing a magneto-electric current in the coils of wire, M M, which are connected up to the line. The strength of this induced current varies continuously, "as nearly as may be, in simple proportion to the velocity of a particle of air engaged in constituting the sound." It travels along the line, and passes through the receiver at the distant station, evoking there the sounds which gave

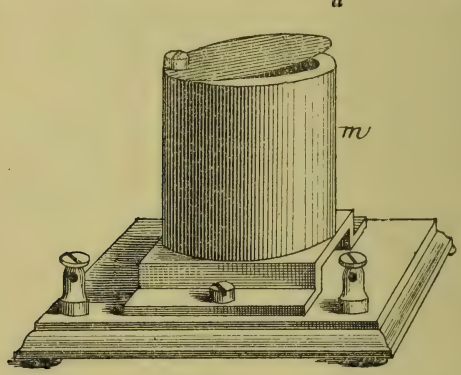


Fig. 18.

rise to it. The receiver is even more simple than the sender, and its action is the reverse. It consists of a tubular electro-magnet, *m*, encased in iron to concentrate its power as much as possible on the circular disk armature, *a*, which is so fixed as to be free to vibrate over its cavity. It is connected up in circuit with the line by the terminals shown. The induced current coming from the sending station passes through the coil of this electro-magnet, and sets the thin disk armature into sonorous vibrations, which are distinctly and clearly heard as a reproduction of the original sounds.

In his address to the physical section of the British Association at Glasgow last year, Sir William Thomson thus

related his personal experience of this speaking telegraph at the Centennial Exhibition, Philadelphia. "In the Canadian department I heard 'To be, or not to be . . . there's the rub,' through an electric wire; but scorning monosyllables, the electric articulation rose to higher flights, and gave me passages taken at random from the New York newspapers: 'S.s. *Cox* has arrived' (I failed to make out the s.s. *Cox*); 'The City of New York,' 'Senator Morton,' 'The Senate has resolved to print a thousand extra copies,' 'The Americans in London have resolved to celebrate the coming Fourth of July.' All this my own ears heard spoken to me with unmistakeable distinctness by the then circular disc armature of just such another little electromagnet as this I hold in my hand." Since then, Professor Bell has been perfecting his apparatus, and we find, on the authority of the "Scientific American," that he has recently achieved even more remarkable success. At a lecture which he delivered at Salem, Mass., a reporter of the "Boston Daily Globe" sent a verbal report of the lecture to the office of his paper in Boston, eighteen miles distant, by means of the telephone. Those reciving the message at Boston even heard, from time to time, the applause of the audience attending the lecture. From the platform Professor Bell spoke to his associate Mr. Watson in Boston. The latter then sent a telegraph message in musical notes, and also a tune from an organ, which were distinctly audible to the audience. On being asked for a song, Mr. Watson complied with "Auld Lang Syne," and finally made a speech, the words of which were heard by all present in the hall, as the applause testified. Mr. Watson then returned thanks, and the meeting ended, we are told, by those at Salem joining in the national anthem, "Hail, Columbia," with those at Boston. In view of these striking facts, it is hardly going too far to anticipate the time when, from St. James's Hall as a centre, Mr. Gladstone will be able to speak to the ears of the whole nation, collected at a hundred different towns, on Bulgarian atrocities, or some other topic of burning interest. Nor need we despair of seeing Herr Wagner from his throne at Bayreuth, dispensing the "music of the future" in one monstre concert to St. Petersburg, Vienna, London, New York—in short, to all the musical world at once.

V. ON THE PRESENT CONDITION OF CHILE.

THOUGH Chile has only had a settled Government for some twenty-five years, the country has now been autonomous for about half a century, and it will, therefore, not be uninteresting at the present time to consider briefly the results which have been achieved in that limited period. With this end in view we propose in this paper to abridge some notes from an exhaustive report, recently furnished to the Foreign Office by Her Majesty's Secretary of Legation at Santiago de Chile, on the progress and general condition of the Republic—a report which reflects great credit on its writer, and, apart from a few defects in the arrangement of its details, is one of the most complete that we remember to have met with in a long experience of such like documents.

We need not dwell at great length on Mr. Rumbold's introductory remarks on the geographical position and physical configuration of the country, for they are more or less familiar to all; but we may mention that Chile claims to extend from the 24th degree of southern latitude to Cape Horn, with a coast-line ranging over 2000 miles, the greater part of her territory—from the province of Aconcagua southwards—being describable as one broad valley running due north and south, with narrower lateral and intersecting valleys, each of which rises step-like above the other to the foot of the giant wall of the Andes. However genial the climate and fertile the soil, the extent of land available for cultivation is necessarily limited by the large proportion of hills and rocks, and by the extensive desert tracts of the northern districts. At the same time the natural declivity from the mountain to the ocean, distinctive of the whole country, as well as its inconsiderable breadth (nowhere much exceeding 120 miles), greatly facilitate communication with the coast at all points. It is thus marked out by Nature for easy exportation of its own produce, and is equally convenient of access to the sea-borne produce of other nations. The Chileans would therefore seem destined to become both an important agricultural and an important commercial and maritime community—points of resemblance with ourselves which they no doubt include in their claim to be considered "the English of South America." Mr. Rumbold does not

profess to make any attempt at a detailed description of Chile, but briefly classes it in the following three main divisions :—

1. The large northern provinces of Atacama and Coquimbo, the former of which is almost entirely made up of sandy deserts replete with mineral wealth. Its richest mines are to be found between Caldera and Mejillones de Bolivia, over a district covering 240 miles. Here are the silver mines of Puquios, Tres Puntas, Chañarcillo, Chimbero, and numerous others, and here, too, are the newly-discovered mines of La Florida. In this vast region every conceivable mineral product is found ; silver in abundance, and copper ; gold, iron, lead, nickel, and cobalt ; not to mention great *pampas* or stretches covered with nitrate of soda, rock-salt, and borax, these latter being of recent discovery, and when analysed found to contain a large proportion of iodine. The great discoveries made at Caracoles—which, it may be well to note, is not in Chilean but in Bolivian territory—have somewhat diverted public attention from the mines of Atacama, but their wealth is none the less remarkable, and it will suffice to say of them that in the thirty years period 1843 to 1873 they yielded over 200,000,000 dollars' worth of mineral produce, or an average annual yield of about £1,320,000. The southernmost portion of the province (the department of Freirina) bears a different aspect from the rest, for it contains the Vale of Huasco, which is renowned as one of the best-cultivated districts of Chile, but upon which the desert is now said to be encroaching at an alarming rate. The department is, above all, the greatest copper district in the world, and its rich mines of Carrizal are well known. The province of Coquimbo, too, abounds in this metal.

2. The limits of the second, or central, division of Chile would be best marked by the Rio Illapel, or Choapa, and the Rio Itata. It comprises the provinces of Aconcagua, considered the garden of Chile ; Santiago, with the large and handsome capital city of the same name ; Valparaiso, with the largest commercial emporium of the South Pacific ; Colcagua, Talca, Maule, and Nuble. It is the heart and soul of the whole country, containing its largest estates and most important towns, yielding its most abundant produce, and supporting the bulk of its population. It is distinctly agricultural, as opposed to the mining north and to the wilder and more pastoral south.

3. The third, or austral, division of Chile, *el Sur*, commences—according to Mr. Rumbold—at the Rio Itata, and

includes the province of Concepcion, the so-called province of Arauco, and the provinces of Valdivia, Llanquihué, and Chiloé. This extensive region is but thinly peopled, and only in part subdued, the greater portion of Arauco, and much of Valdivia and Llanquihué, being still in the hands of the independent aboriginal Araucanian tribes that held them before the Conquest. It is a region eminently adapted for stock-breeding and other pastoral pursuits, but it also includes large coal-fields. Its moist and temperate climate, excellent soil, numerous and commodious harbours, beautiful rivers, with banks clothed down to the water's edge by primæval forests full of valuable timber, point it out as especially favourable to emigration from Europe.

Before quitting this part of our subject, a few words must be devoted to the colony of Punta Arenas, or Sandy Point, in the Straits of Magellan, which is the most southern civilised community of the globe. The station is described on authority as peculiarly healthy; the mean temperature of the whole year is 44·8° F.; that of spring being 45·9°; of summer, 52·6°; of autumn, 44·6°; and of winter, 35·8°. The settlement now produces enough to support itself; its shores are covered with forests of Antarctic beech, and it is expected that a large and profitable trade in railway-sleepers and other timber may be developed with the countries on the River Plate: it likewise contains coal-mines, which promise well, and which may not impossibly open out an important future to the place as a coaling-station; mines of copper are said to exist, and gold is found in the Rio de la Mina to the north of the settlement; but the sources of wealth more special to the colony are its cod-fisheries and trade in guanaco skins, and in ostrich skins and feathers. In 1868 the population was 195, and in 1875 was 1144.

Census.—According to the preliminary Report of the Commissioners the population actually found in Chile on April 19, 1875, numbered 2,068,424 souls, being an increase of 249,201, or 13·7 per cent, in ten years. The difficulty of counting a people thinly spread over an immense territory, and in many cases disinclined to help the persons charged with the operation, was such that the Census Commissioners think it safe to add at least 10 per cent for omissions, and they accordingly put the total figure of the population at 2,319,266. Some curiosities of the census are worth noting. As instances of the vague distrust with which the process was regarded in many rural districts, it is stated that at Conchali, in the department of Quillota, nearly all the *peones*, or labourers, on one estate left their huts on the eve

of the 19th of April, and took to the hills for refuge. At Romeral a similar exodus occurred, and only 500 inhabitants were found instead of at least 1200, known to belong to the place. The prevailing idea appeared to be that a forced *levée en masse* was intended, or that the population was being counted with a view to the imposition of a poll-tax. As an illustration, on the other hand, of the little intelligence sometimes shown in collecting the necessary data, it is said that in one district the enumerators positively refused to note down any children under nine years of age. The number of foreigners in the country appears small when the large foreign trade is considered. The census puts them at 26,528, chiefly resident at Valparaiso, Santiago, and the mining capital of Copiapó. In reality, however, the foreign element in Chile is much larger than it would seem to be, for all children of foreigners born in the country are accounted Chileans. These and the offspring of mixed marriages together produce a considerable admixture of foreign blood not without its uses in so young a community. It is worthy of note, too, that these half-breeds, as a rule, show a marked attachment for the country of their birth. The census roughly estimated the independent Indian population at 44,000, 20,000 of whom are accredited to the debateable regions of Patagonia and Tierra del Fuego, and 24,000 to Araucania proper, but the last-mentioned figures are believed to be much below the mark.

Education.—The progress of late years in this direction has been not inconsiderable. In 1874 the Report of the Minister of Public Instruction showed that there were 806 public elementary schools, in which 62,244 scholars of both sexes were inscribed, and 478 private schools with 23,198 scholars. Altogether, therefore, there were throughout the Republic 1284 schools; in which 85,442 children were assumed to be receiving some degree of education; the proportion thus obtained being 1 scholar out of 4·94 children of educational age, or not far short of double what it was twenty years before. The cause of general education has received considerable attention at the hands of the Chilean Government in the last twenty-five years, but, from what Mr. Rumbold states, it would seem that it has been less warmly taken up by the more influential classes of the country. This, however,—it is only fair to add—finds some excuse in the stolid indifference of the peasantry, which is such as a severe compulsory system could probably alone overcome.

Post Office.—Closely allied to the development of education

are the postal statistics of the country, which may fitly claim a passing word. In 1852 71,168 letters, newspapers, and documents of all kinds passed through the post. In twelve years, that is in 1864, their number had risen to 4,375,408, while in 1874 it reached the figure 12,984,428, the increase in 1874 over the previous year being no less than 1,748,800. In 1874 there were transmitted 141,897 telegrams, for which 58,295 dollars were received, while, in 1875, the number rose to 161,459, and produced 59,866 dollars. The Transandine telegraph line of Messrs. Clark, which has been working for some years, is an achievement of which any country might well be proud; the wires are conveyed across the mountains on stone pillars, and in the more exposed portions carried underground.

Public Revenue and Expenditure.—The ordinary revenue for 1874 was £3,080,164, and the extraordinary revenue £52,180, or a total of £3,162,344, while the ordinary expenditure was £3,431,017, and the extraordinary expenditure £1,070,735, or a total of £4,501,772. The deficit on the year was therefore £1,369,428, of which £897,692 were taken out of the loan contracted in England in the previous year, and the remainder was met out of Treasury balances, and by a temporary advance from the National Bank of Chile. In this connection it may be remarked that the works of public utility undertaken in the last few years are of considerable magnitude, and though, for the most part, undoubtedly destined to be remunerative, the outlay incurred on them has sensibly disturbed the equilibrium of Chilean finance. For information respecting the Chilean loans, and the general indebtedness of the State, we must refer the reader to Mr. Rumbold's Report.

General Review of Exports.—Agricultural.—The Chilean exports, with the trifling exception of a few local manufactures and miscellaneous articles, altogether not amounting to one per cent of the whole, are made up of the mining produce and the agricultural produce (raw and manipulated) of the country in nearly equal proportions: but it is as a mining country that Chile is of absolute interest and importance to foreign markets.

A few notes with regard to the rise of Chilean agriculture will not be without interest. Up to the year 1848 Chile grew little beyond what she consumed, and the production of cereals was not looked upon as lucrative. The discovery of gold in California, in 1847, changed the whole aspect of things. The exports in wheat and flour alone increased nearly fourfold in two years, and sevenfold in seven. This

happy state of things could not, of course last for long, and from various causes the trade with California collapsed as suddenly as it had arisen. The exports to that country which in 1850 had reached 2,445,868 dollars (upwards of 1,500,000 in flour and wheat alone), and which made up nearly one-half of the entire exports for the year, had fallen to 275,763 dollars in 1855, and further fell to 178,484 dollars in 1858; the breadstuffs exported to San Francisco in the latter year only amounting to the insignificant sum of 15,000 dollars. But at the very time when the Californian "Eldorado" was eluding the grasp of the Chilean producers, a fresh but likewise transient outlet was opened to them by the Australian gold discoveries. This trade only lasted from 1853 to 1859, but in 1855 the exports represented no less a sum than 2,698,911 dollars, of which 2,541,692 dollars, or £508,000, was the value of the wheat and flour. Meanwhile equally remote and unforeseen causes were combining to assure to Chile a valuable and far steadier customer in Peru, which, in the last century sent wheat to Chile, and raised an abundance of grain, cattle, potatos, and other kinds of food, but had by degrees neglected her production, and taken to tropical husbandry, such as cotton and sugar-cane planting. The war in the United States for a time made cotton planting so lucrative as to turn Peruvian capital yet more in that direction. Thus Chile, which from the first year of which her statistics furnish any record, had supplied her neighbour with an average value of 250,000 dollars of breadstuffs, now found an outlet for five times that amount. The general conditions of the trade with Peru have been completely reversed in the last thirty years, as will be seen from the following figures:—

	1845. Dollars.	1863. Dollars.	1874. Dollars.
Imports from Peru . .	1,474,880	701,297	1,947,770
Exports to ,,	674,552	2,619,386	6,016,413

The development of the export trade with England has been even more surprising, for in 1874 the breadstuffs sent to this country had reached 6,457,945, the general exports in the same year figuring for 22,259,730 dollars.

Mineral Exports.—Passing over the trade in live stock, and a few agricultural productions of comparatively minor importance, we must devote a brief space to the mineral produce of the country. In the thirty years between 1844 and 1873 this amounted in value to the large sum of £73,888,018, from which, however, we ought to subtract

£4,082,195, the value of the gold and silver coin and guano exported, that of the latter amounting to about £105,000. The mineral exports doubled between 1844 and 1852 (rising from 3,618,987 dollars to 7,807,106), up to which last named year the mines of Atacama and Coquimbo had been but partially worked. In 1857 they had doubled again, and have since gone on increasing in value, with some variations, but not in the same rapid proportion. The mining province of Atacama, and, in particular, the district of Copiapo, from which nearly two-thirds of the Chilean mineral produce are derived, is said to show of late marked signs of exhaustion. In the most northern district of this province is situated the group of silver mines known as La Florida. These mines, which are of recent discovery, extend over a zone some twelve kilometres, and according to the latest accounts are of exceeding richness. At the great copper-smelting works at Guayacan the monthly out-turn of bar-copper exceeds £60,000 in value; and of nearly equal importance are the smelting works at Lota, south of Concepcion, nominally belonging to the Lota Coronel Company, but in reality the property of the Cousiño family. At Lota, too, the same family owns the largest coal mines as yet worked in Chile. Coal is found in many parts, especially in the southern coast districts, but unfortunately owing to its inferior quality, and to the heavy cost of extraction, the Chile coal must, for some years to come, compete at a great disadvantage with that brought from Great Britain at nominal freights by sailing vessels in quest of return cargoes. The value of the coal exported between 1844 and 1873 was about £1,064,200.

Concluding Remarks.—Mr. Rumbold not unfairly expresses a hope that the observations contained in his Report will convey the notion of a sober-minded, practical, laborious, well-ordered, and respectably-governed community, standing out in great contrast to the other States of kindred origin and similar institutions spread over the South-American continent. The blessings which Chile enjoys she owes to the pure traditions implanted in her administration by the founders of the Republic; to the preponderating share taken in public affairs by the higher and wealthier class; to the happy eradication of militarism; to the nearly entire absence of those accidental sources of wealth (gold, guano, and nitrate) so lavishly bestowed by Providence on some of her neighbours; to the consequent necessity for strenuous labour rapidly repaid by a bountiful soil; above all, perhaps,

to the neglect of her former masters, which, when she had cast off the yoke, drove her to create everything for herself, and called forth exceptional energies in the nation. Most of these may be summed up in two words—work and shrewd sense (*trabajo i cordura*).

Thanks largely to the natural advantages of their country, and not a little too to foreign—mostly English—energy and assistance, the Chilean people have now attained a remarkable degree of prosperity; but Mr. Rumbold points out that they have of late shown signs of the intoxicating effects of good fortune, and this is exhibited in the fact that they are bent (the Government and upper classes setting the example) rather on decorating and beautifying their house than on setting it in more perfect order. A first visit to the city of Santiago, he says, cannot but be matter of agreeable surprise to an intelligent European; but after a more lengthened stay the ambitious growth and luxury of the town will probably seem to him out of due proportion to the power and resources of the country of which it is the capital. One is, indeed, scarcely prepared to find 90 miles inland, at the foot of the Andes, a city of some 160,000 inhabitants, with such handsome public bulidings, stately dwelling-houses, and exceptionally fine promenades. On this subject the reader will find, in Mr. Rumbold's report, some further particulars of interest, to which want of space alone precludes us from referring in greater detail.

To conclude:—The opening years of the century were wont to see an anxious crowd watching at Valparaiso for the first sight of the ship which, at intervals during the year, was sent down from Peru to that port with supplies and luxuries commensurate with the wants of a needy outlying province. The same port saw close upon 3000 vessels —one-third of them steamers—entering and leaving it in 1874, while over 11,600 vessels traded in the year in all the harbours of the Republic. In the same year it was reckoned that the value of the capital in land throughout the country amounted to 666,000,000 dollars, or about £133,200,000, while the capital invested in banks, or in general commercial and industrial operations, might be safely estimated at over £30,000,000. Figures like these speak eloquently of the progress achieved in the three-quarters of a century that have elapsed since men gathered on the beach to wait the advent of the promised ship, and on the facts they reveal Chile might well rest a claim to universal regard and sympathy. But she can afford to rest that claim on far higher

grounds. She may fairly point to the pefectly pure, albeit somewhat dilatory, action of her courts of justice, the integrity of her administration, the general and willing obedience of her people to the law, and to the good sense of a community where men are content to busy themselves with their own concerns in preference to those of the State, and choose to live on the labour of their hands and brains rather than on the loaves and fishes of the public treasury. In these things Chile, adds Mr. Rumbold, stands nearly alone among South-American States, and accordingly should be classed apart from them.

NOTICES OF BOOKS.

Mesmerism, Spiritualism, &c., Historically and Scientifically Considered. Being Two Lectures delivered at the London Institution, with Preface and Appendix. By WILLIAM B. CARPENTER, C.B., M.D., F.R.S., &c., &c. London: Longmans, Green, and Co., 1877.

THE two lectures which Dr. Carpenter gave last year at the London Institution were generally reported by the press and led to some controversy. They were then published in Fraser's Magazine; and they are now re-published with what are considered to be *pièces justificatives* in an appendix. We may therefore fairly assume that the author has here said his best on the subject—that he has carefully considered his facts and his arguments—and that he can give, in his own opinion at least, good reasons for omitting to notice certain matters which seem essential to a fair and impartial review of the whole question.

Dr. Carpenter enjoys the great advantage, which he well knows how to profit by, of being on the popular side, and of having been long before the public as an expounder of popular and educational science. Everything he writes is widely read; and his reiterated assurances that nobody's opinion and nobody's evidence on this particular subject is of the least value unless they have had a certain *special early training* (of which, it is pretty generally understood, Dr. Carpenter is one of the few living representatives) have convinced many people that what he tells them must be true and should therefore settle the whole matter. He has another advantage in the immense extent and complexity of the subject and the widely scattered and controversial nature of its literature. By ranging over this wide field and picking here and there a fact to support his views and a statement to damage his opponents, Dr. Carpenter has rendered it almost impossible to answer him on every point, without an amount of detail and research that would be repulsive to ordinary readers. It is necessary therefore to confine ourselves to the more important questions, where the facts are tolerably accessible and the matter can be brought to a definite issue; though, if space permitted, there is hardly a page of the book in which we should not find expressions calling for strong animadversion, as, for example, the unfounded and totally false general assertion at p. 6, that "believers in spiritualism make it a reproach against men of science that they entertain a prepossession in favour of the ascertained and universally admitted laws of nature." Vague general assertions of this kind, without a particle of proof offered or which can be offered, are alone suffi-

cient to destroy the judicial or scientific claims of the work; but we have no intention of wasting space in further comment upon them.

Dr. Carpenter lays especial stress on his character of historian and man of science in relation to this enquiry. He parades this assumption in his title page and at the very commencement of his preface. He claims therefore to review the case as a judge, giving full weight to the evidence on both sides, and pronouncing an impartial and well-considered judgment. He may, indeed, believe that he has thus acted—for dominant ideas are very powerful—but any one tolerably acquainted with the literature and history of these subjects for the last thirty years, will most assuredly look upon this book as the work of an advocate rather than of a judge. In place of the impartial summary of the historian he will find the one-sided narrative of a partisan; and, instead of the careful weighing of fact and experiment characteristic of the man of science, he will find loose and inaccurate statements, and negative results set up as conclusive against positive evidence. We will now proceed to demonstrate the truth of this grave accusation, and shall in every case refer to the authorities by means of which our statements can be tested.

The first example of Dr. Carpenter's "historical" mode of treating his subject which we shall adduce, is his account (pp. 13 to 15) of the rise of mesmerism in this country owing to the successful performance of many surgical operations without pain during the mesmeric trance. Dr. Carpenter writes of this as not only an admitted fact, but (so far as any word in his pages shows), as a fact which was admitted from the first, and which never went through that ordeal of denial, misrepresentation, and abuse by medical men and physiologists that other phenomena are still undergoing from a similar class of men. Yet Dr. Carpenter was in the thick of the fight and must know all about it. He must know that the greatest surgical and physiological authorities of that day—Sir Benjamin Brodie and Dr. Marshall Hall—opposed it with all the weight of their influence, accused the patients of imposture, or asserted that they might be "naturally insensible to pain," and spoke of the experiments of Dr. Elliotson and others as "trumpery," and as "polluting the temple of science." He must know, too, that Dr. Marshall Hall professed to demonstrate "physiologically" that the patients were impostors, because certain reflex-actions of the limbs which he declared ought to have occurred during the operations did not occur. The medical periodicals of the day were full of this, and a good summary may be found in Dr. Elliotson's "Surgical Operations without Pain, &c.," London, 1843. Dr. Carpenter tells us how his friends, Dr. Noble and Sir John Forbes, in 1845 accepted and wrote in favour of the reality of the facts; but it was hardly "historical" to tell us this as the whole truth, when, for several years previously, the most violent controversy, abuse, and even

persecution, had raged on this very matter. Great physiological authorities were egregiously in the wrong then, and the natural inference to those who know the facts is, that other physiological authorities who now deny equally well attested facts may be no more infallible than their predecessors.

Dr. Carpenter persistently denies that there is any adequate evidence of the personal influence of the mesmeriser on the patient independent of the patient's knowledge and expectation, and he believes himself to be very strong in the cases he adduces, in which this power has been tested and failed. But he quite ignores the fact that all who have ever investigated the higher phases of mesmerism—such as influence at a distance, community of sensation, transference of the senses, or true clairvoyance—agree in maintaining that these phenomena are very uncertain, depending greatly on the state of body and mind of the patient, who is exceedingly susceptible to mental impressions, the presence of strangers, fatigue, or any unusual conditions. Failures continually occur, even when the mesmeriser and patient are alone or when only intimate friends are present; how, then, can the negative fact of a failure before strangers and antagonists prove anything? Dr. Carpenter also occupies his readers' attention with accounts of hearsay stories which have turned out exaggerated or incorrect, and lays great stress on the "disposition to overlook sources of fallacy," and to be "imposed on by cunning cheats" which this shows. This may be admitted; but it evidently has no bearing on well-authenticated and carefully observed facts, perfectly known to every student of the subject. Our author maintains, however, that such facts do not exist, and that "the evidence for these higher marvels has invariably broken down when submitted to the searching tests of trained experts." Here the question arises, who are "trained experts?" Dr. Carpenter would maintain that only sceptical medical men and professed conjurors deserve that epithet, however ignorant they may be of all the conditions requisite for the study of these delicate and fluctuating phenomena of the nervous system. But we, on the contrary, would only give that name to enquirers who have experimented for months or years on this very subject, and are thoroughly acquainted with all its difficulties. When such men are also physiologists it is hardly consistent with the historical and scientific method of enquiry to pass their evidence by in silence. I have already called Dr. Carpenter's attention to the case of the lady residing in Professor Gregory's own house, who was mesmerised at several streets distance by Mr. Lewis without her knowledge or expectation. This is a piece of direct evidence of a very satisfactory kind, and outweighs a very large quantity of negative evidence; but no mention is made of it except the following utterly unjustifiable remark:—"His (Mr. Lewis's) utter failure under the scrutiny of sceptical enquirers, obviously discredits all his previous statements, except

to such as (like Mr. A. R. Wallace, who has recently expressed his full faith in Mr. Lewis's self asserted powers) are ready to accept without question the slenderest evidence of the greatest marvels." ("Mesmerism, Spiritualism, &c.," p. 24.) Now will it be believed that this statement, that I "place full faith in Mr. Lewis's *self-asserted powers*," has not even the shadow of a foundation. I know nothing of Mr. Lewis or of his powers, self-asserted or otherwise, but what I gain from Prof. Gregory's account of them; and in my letter to the "Daily News," immediately after the delivery of Dr. Carpenter's lectures, I referred to this account. I certainly have "full faith" in Professor Gregory's very careful narrative of a fact entirely within his own knowledge. This may be "the slenderest evidence" to Dr. Carpenter, but slender or not he chooses to evade it, and endeavours to make the public believe that I and others accept the unsupported assertions of an unknown man. It is impossible adequately to characterise such reckless accusations as this without using language which I should not wish to use. Let us pass on, therefore, to the evidence which Dr. Carpenter declares to be fitly described as "the slenderest." M. Dupotet, at the Hotel de Dieu, in Paris, put a patient to sleep when behind a partition, in the presence of M. Husson and M. Recamier, the latter a complete sceptic. M. Recamier expressed a doubt that the circumstances might produce expectation in the patient, and himself proposed an experiment the next day, in which all the same conditions should be observed, except that M. Dupotet should not come at all till half an hour later. He anticipated that the "expectation" would be still stronger the second time than at first, and that the patient would be mesmerised. But the result was quite the reverse. Notwithstanding every minute detail was repeated as on the previous day when the operator was in the next room, the patient showed no signs whatever of sleep either natural or somnambulic (Teste's "Animal Magnetism," Spillan's Translation, p. 159). The Commission appointed by the Academie Royale de Medicine in 1826 sat for five years and investigated the whole subject of animal magnetism. It was wholly composed of medical men, and in their elaborate report, after giving numerous cases, the following is one of their conclusions:—

"14. We are satisfied that it (magnetic sleep) has been excited under circumstances where those magnetised could not see, and were entirely ignorant of the means employed to occasion it."

These were surely "trained experts;" yet they declare themselves satisfied of that, the evidence for which, Dr. Carpenter says, has always broken down when tested.

Baron Reichenbach's researches are next discussed, and are coolly dismissed with the remark that "it at once became apparent to experienced physicians, that the whole phenomena were subjective, and that 'sensitives' like Von Reichenbach's can

feel, see, or smell anything they were led to believe they *would* feel, see, or smell." His evidence for this is, that Mr. Braid could make his subjects do so, and that Dr. Carpenter had seen him do it. One of them, for instance,—an intellectual and able Manchester gentleman,—"could be brought to see flames issuing from the poles of a magnet of any form or colour that Mr. Braid chose to name." All this belongs to the mere rudiments of mesmerism and is known to every operator. Two things, however, are essential—the patient or sensitive must be, or have been, mesmerised, or electro-biologised as it is commonly called, and the *suggestion* must be actually made. Given these two conditions and no doubt twenty persons may be made to declare that they see green flames issuing from the operator's mouth; but no single case has been adduced of persons in ordinary health, not subject to any operation of mesmerism, &c., being all caused to see this or any other thing in agreement, by being merely brought into a dark room and asked to describe accurately what they saw. Yet this is what Von Reichenbach did, and much more. For, in order to confirm the evidence of the "sensitives" first experimented on, he invited a large number of his friends and other persons in Vienna to come to his dark room, and the result was that about *sixty persons* of various ages and conditions saw and described exactly the same phenomena. Among these were a number of literary, official, and scientific men and their families, persons of a status fully equal to that of Dr. Carpenter and the Fellows of the Royal Society—such as Dr. Nied, a physician; Professor Endlicher, director of the Imperial Botanic Garden; Chevalier Hubert von Rainer, barrister; Mr. Karl Schuh, physicist; Dr. Ragsky, Professor of Chemistry; Mr. Franz Kollar and Dr. Diesing, Curators in the Imperial Natural History Museum, and many others. There was also an artist, Mr. Gustav Anschütz, who could see the flames, and drew them in their various forms and combinations. Does Dr. Carpenter really ask his readers to believe that his explanation applies to these gentlemen? That they all quietly submitted to be told *what* they were to see, submissively *said* they saw it, and allowed the fact to be published at the time, without a word of protest on their part from that day to this? But a little examination of the reports of their evidence shows that they did not follow each other like a flock of sheep, but that each had an individuality of perceptive power, some seeing one kind of flame better than another; while the variety of combinations of magnets submitted to them, rendered anything like suggestion as to what they were to see quite impossible, unless it were a deliberate and wilful imposture on the part of Baron von Reichenbach.

But again, Dr. Carpenter objects to the want of tests, and especially his pet test of using an electro-magnet, and not letting the patients know whether the electric circuit which "makes" and "unmakes" the magnet was complete or broken. How

far this test, had it been applied, would have satisfied the objector, may be imagined from his entirely ignoring all the tests, many of them at least as good, which were actually applied. The following are a few of these:—Test 1. Von Reichenbach arranged with a friend to stand in another room with a stone wall between him and the patient's bed, holding a powerful magnet, the armature of which was to be closed or opened at a given signal. The patient detected, on every occasion, whether the magnet was opened or closed. Test 2. M. Baumgartner, a professor of physics, after seeing the effects of magnets on patients, took from his pocket what he said was one of his most powerful magnets, to try its effects. The patient, to Von Reichenbach's astonishment, declared she found this magnet on the contrary very weak, and its action on her hardly more perceptible than a piece of iron. M. Baumgartner then explained that this magnet, though originally very powerful, had been as completely as possible deprived of its magnetism, and that he had brought it as a test. Here was *suggestion* and *expectation* in full force, yet it did not in the least affect the patient. (For these two tests see "Ashburner's Translation of Reichenbach," pp. 39, 40.) Test 3. A large crystal (placed in a new position before each patient was brought into the dark room) was always at once detected by means of its light, yellower and redder than that from magnets (*loc. cit.*, p. 86). Test 4. A patient confined in a darkened passage held a wire which communicated with a room in which experiments were made on plates connected with this wire. As these plates were exposed to sunlight or shade, the patient described corresponding changes in the luminous appearances of the end of the wire (*loc. cit.* p. 147). Test 5. The light from magnets, &c., was thrown on a screen by a lens, so that the image could be instantly and noislessly changed in size and position at pleasure. Twelve patients, eight of them healthy and new to the enquiry, saw the image, and described its alterations of size and position as the lens or screen was shifted in the dark (*loc. cit.*, p. 585). Dr. Carpenter's only reply to all this is, that "Baron Reichenbach's researches upon 'Odyle' were discredited a quarter of a century ago, alike by the united voice of scientific opinion in his own country, and by that of the medical profession here." Even if this were the fact, it would have nothing to do with the matter, which is one of experiment and evidence, not of the belief or disbelief of certain prejudiced persons, since to *discredit* is not to *disprove*. The painless operations in mesmeric sleep were "discredited" by the highest medical authorities in this country, and yet they were true. But Dr. Elliotson, Dr. Ashburner, and others, accepted Reichenbach's discoveries; and some of the Vienna physicians even, after seeing the experiments with persons "whose honour, truthfulness, and impartiality they could vouch for," also accepted them as proved.

The facts of the luminosity of magnets was also independently established by Dr. Charpignon, who, in his "Physiologie, Médicine, et Metaphysique du Magnetisme," published in 1845—the very same year in which the account of Von Reichenbach's observations first appeared—says: "Having placed before the sonnambulists four small bars of iron, one of which was magnetised by the loadstone, they could always distinguish this one from the others, from its two ends being enveloped in a brilliant vapour. The light was more brilliant at one end (the north pole) than at the other. I could never deceive them; they always recognised the nature of the poles, although when in their normal state they were in complete ignorance of the subject." Surely here is a wonderful confirmation. One observer in France and another in Germany make the same observation about the same time, and quite independently; and even the detail of the north pole being the more brilliant agrees with the statement of Reichenbach's sensitives (Ashburner's Trans., p. 20).

Our readers can now judge how far the historic and scientific method has been followed in Dr. Carpenter's treatment of the researches of Von Reichenbach, not one of the essential facts here stated (and there are hundreds like them) being so much as alluded to, while "suggestion," expectation," and "imposture," are offered as fully explaining everything. We cannot devote much time to the less important branches of the subject, but it is necessary to show that *in every case* Dr. Carpenter misstates facts and sets negative above positive evidence. Thus, as to the magnenometer* and odometer of Mr. Rutter and Dr. Mayo, all the effects are imputed to expectation and unconscious muscular action, and we have this positive statement: "It was found that the constancy of the vibrations depended entirely upon the operator's watching their direction, and, further, that when such a change was made *without the operator's knowledge* in the conditions of the experiment, as *ought*, theoretically, to alter the direction of the oscillations, no such alteration took place." Yet Mr. Rutter clearly states—1. That the instrument can be affected through the hand of a *third* person with exactly the same result (Rutter's "Human Electricity," App., p. 54). 2. That the instrument is affected by a crystal on a *detached stand* brought close to the instrument, but without contact (*loc. cit.*, p. 151). 3. That many persons, however "expectant" and anxious to succeed, have no power to move the instrument. 4. That substances *unknown to the operator*, and even when held by a third party caused correct indications, and that an attempt to deceive by using a substance under a wrong name was detected by the movements of the instrument (*loc. cit.*, Appendix, p. lvi.). Here then Mr. Rutter's posi-

* The magnenometer is a delicate pendulum which, when its support is touched by certain persons, vibrates in a definite direction, the direction changing on the motion suddenly stopping when different substances are touched at the same time by the operator.

tive testimony is altogether ignored, while the negative results of another person are set forth as conclusive. Next we have the evidence for the divining-rod similarly treated. Dr. Mayo is quoted as supporting the view that the rod moved in accordance with the "expectations" of the operator, but on the preceding page of Dr. Mayo's work, other cases are given in which there was no expectation; and the fact that Dr. Mayo was well aware of this source of error, and was a physiologist and physician of high rank, entitles his opinion as to the reality of the action in other cases to great weight. Again, we have the testimony of Dr. Hutton, who saw the Hon. Lady Milbanke use the divining-rod on Woolwich Common, and who declares that it turned where he knew there was water, and that in other places where he believed there was none it did not turn: that the lady's hands were closely watched, and that no motion of the fingers or hands could be detected, yet the rod turned so strongly and persistently that it became broken. No other person present could voluntarily or involuntarily cause the rod to turn in a similar way (Hutton's "Mathematical Recreations," Ed. 1840, p. 711). The evidence on this subject is most voluminous, but we have adduced sufficient to show that Dr. Carpenter's supposed demonstration does not account for all the facts.

We now come to the very interesting and important subject of clairvoyance, which Dr. Carpenter introduces with a great deal of irrelevant matter calculated to prejudge the question. Thus, he tells his readers that "there are at the present time numbers of educated men and women who have so completely surrendered their 'common sense' to a dominant prepossession as to maintain that any such monstrous fiction (as of a person being carried through the air in an hour from Edinburgh to London) ouught to be believed, even upon the evidence of a single witness, if that witness be one upon whose testimony we should rely in the ordinary affairs of life!" He offers no proof of this statement, and we venture to say he can offer none, and it is only another example of that complete misrepresentation of the opinions of his opponents with which this book abounds. At page 71, however, we enter upon the subject itself, and at once encounter one of those curious examples of ignorance (or suppression of evidence) for which Dr. Carpenter is so remarkable in his treatment of this subject. We have been already told (p. 11) of the French Scientific Commission which about a hundred years ago investigated the pretensions of Mesmer, and decided, as might have been anticipated, against him. Now, we have the statement that "it was by the French Academy of Medicine, in which the mesmeric state had been previously discussed with reference to the performance of surgical operations, that this new and more extraordinary claim (*clairvoyance*) was first carefully sifted, in consequence of the offer made in 1837 by M. Burdin of a prize of 3000 francs to anyone who should

be found capable of reading through opaque substances." The result was negative. No clairvoyant succeeded under the conditions imposed. The reader unaccustomed to Dr. Carpenter's historical method would naturally suppose this statement to be correct, and that *clairvoyance* was *first* carefully sifted in France *after* 1837, though he might well doubt, if offering a prize for reading under rigid conditions was an adequate means of sifting a faculty so eminently variable, uncertain, and delicate as clairvoyance is admitted to be. What, then, will be his astonishment to find that this same "Académie Royal de Medicine" had appointed a commission of eleven members in 1826, who inquired into the whole subject of mesmerism for *five years*, and in 1831 reported in full, and *in favour* of the reality of almost all the alleged phenomena, *including clairvoyance*. Of the eleven members, nine attended the meetings and experiments, and all nine signed the report, which was therefore unanimous. This report, being full and elaborate, and the result of personal examination and experiment by medical men—the very "trained and sceptical experts," who are maintained by Dr. Carpenter to be the only adequate judges—is wholly ignored by him. In this report we find among the conclusions—"24. We have seen two somnambulists distinguish with their eyes shut objects placed before them: name cards, read books, writing, &c. This phenomenon took place even when the opening of the eyelids was accurately closed by means of the fingers."* Is it not strange that the "historian" of mesmerism, &c., should be totally ignorant of the existence of this report, which is referred to in almost every work on the subject? Yet he must be thus ignorant or he could never say, as he does in the very same page quoted above (p. 71), "that in every instance (so far as I am aware) in which a thorough investigation has been made into those 'higher phenomena' of mesmerism, the supposed proof has completely failed." It cannot be said that investigation by nine medical men carried on for five years with every means of observation and experiment, and elaborately reported on, was not "thorough," whence it follows that Dr. Carpenter must be ignorant of it, and our readers can draw their own inference as to the value of his opinion, and the dependence to be placed on his scientific and and historical treatment of this subject.

More than twenty-five pages of the book are occupied with more or less detailed accounts of the failures and alleged exposures of clairvoyants, while not a single case is given of a clairvoyant having stood the test of rigid examination by a committee, or by medical or other experts, and the implication is that none such are to be found. But every enquirer knows that clairvoyance is a most delicate and uncertain phenomenon, never to be certainly calculated on, and this is repeatedly stated in the

* Archives Generales de Medecine, vol. xx.; also in LEE'S Animal Magnetism, pp. 13 to 29.

works of Lee, Gregory, Teste, Deleuze, and others. How, then, can any number of individual failures affect the question of the reality of the comparatively rare successes. As well deny that any rifleman ever hit the bull's-eye at 1000 yards, because none can be sure of hitting it always, and at a moment's notice. Several pages are devoted to the failure of Alexis and Adolphe Didier under test conditions in England, ending with the sneering remark, "Nothing, so far as I am aware, has ever been since heard of this *par nobile fratrum*." Would it (to use an established formula) surprise Dr. Carpenter to hear that these gentlemen remained in England a considerable time after the date he alludes to, that they have ever since retained their power and reputation, and that both still practise successfully medical clairvoyance, the one in London, and the other in Paris? To balance the few cases of failure by Alexis, Dr. Lee has given his personal observations of ten times as many successes, some of them of the most startling kind ("Animal Magnetism," pp. 255, 277). We can only find room here for two independent and complete tests. The first is given by Serjeant Cox as witnessed by himself. A party of experts was planned to test Alexis. A word was written by a friend in a distant town and enclosed in an envelope, *without any of the party knowing what the word was*. This envelope was enclosed successively in six others of thick brown paper, each sealed. This packet was handed to Alexis, who placed it on his forehead, and in three minutes and a half wrote the contents correctly, imitating the very handwriting. ("What am I," vol. ii., p. 167.) Now unless this statement by Serjeant Cox is absolutely false, a thousand failures cannot outweigh it. But we have, if possible, better evidence than this; and Dr. Carpenter knows it, because I called his attention to it in the "Daily News." Yet he makes no allusion to it. I refer to the testimony of Robert Houdin, the greatest of modern conjurers, whose exploits *are quoted* by Dr. Carpenter, when they serve his purpose (pp. 76, 111). He was an absolute master of card-tricks, and knew all their possibilities. He was asked by the Marquis de Mirville to visit Alexis, which he did twice. He took his own new cards, dealt them hiself, but Alexis named them as they lay on the table, and even named the trump before it was turned up. This was repeated several times, and Houdin declared that neither chance nor skill could produce such wonderful results. He then took a book from his pocket and asked Alexis to read something eight pages beyond where it was opened at a specified level. Alexis pricked the place with a pin, and read four words, which were found at the place pricked nine pages on. He then told Houdin numerous details as to his son, in some of which Houdin had tried to deceive him, but in vain; and when it was over Houdin declared it "stupefying," and the next day signed a declaration that the report of what took place was correct, adding, "the

more I reflect upon them the more impossible do I find it to class them among the tricks which are the object of my art." The two letters of Robert Houdin were published at the time (May, 1847) in "Le Siècle," and have since appeared in many works, among others in Dr. Lee's "Animal Magnetism" (pp. 163 and 231).

One of the supposed exposures made much of by Dr. Carpenter is that of Dr. Hewes's "Jack," which is suggestive as showing the complete ignorance of many experimenters thirty years ago as to the essential conditions of the manifestation of so delicate and abnormal a faculty as clairvoyance, ignorance shared in by believers and sceptics alike. According to Dr. Carpenter (whose account he informs me is taken from an article by Dr. Noble in the "British and Foreign Medical Review" of April, 1845), Jack's eyes were "bound down by surgeons with strips of adhesive plaster, over which were folds of leather, again kept in place by other plasters." Jack then read off, *without the least hesitation*, everything that was presented to him. But a young Manchester surgeon had his eyes done up in the same manner, and, by *working the muscles of his face* till he had *loosened the plasters*, was enabled to read by *looking upwards*. The conclusion was immediately jumped at that this was the way Jack did it, although no *working of the muscles of the face* had been observed, and no *looking upwards* described. Instead, however, of repeating the experiment under the same conditions, but more watchfully, it was proposed that the *entire eyes should be covered up with a thick coating of shoemakers' wax!* The boy objected and resisted, and it was put on by force; and then, the clairvoyant powers being annihilated, as might have been anticipated, there was great glorification among the sceptics, and Dr. Carpenter indulges himself in a joke, telling us that Jack now "plainly saw, even with his eyes shut, that his little game was up." To any one who considers this case, even as related by Dr. Carpenter, it will be evident that the boy was a genuine clairvoyant. Adhesive plaster properly applied by a medical man on a passive subject, is not to be loosened by imperceptible working of the muscles, and it is too great a demand upon our credulity to ask us to believe that this occurred undetected by the acute medical sceptics watching the whole procedure. We have, however, fortunately, another case to refer to, in which this very test was carried out to its proper conclusion by examining the state of the plaster *after the clairvoyance*, when the alleged looseness could be instantly detected. A clairvoyant boy at Plymouth was submitted to the examination of a sceptical committee, who appear to have done their work very thoroughly. First his eyes were examined, and it was found that the balls were so turned up that even were the eyelids a little apart, ordinary vision was impos-

sible.* Then he was closely watched, and while the eyelids were seen to be perfectly closed, he read easily. Then adhesive plaster was applied, carefully warmed, in three layers, and it was watched to see that the adhesion was perfect all round the edges. Again the boy read what was presented to him, sometimes easily, sometimes with difficulty. At the end of the experiments the plaster was taken off strip by strip by the committee, and it was found to be perfectly secure, and the eyelids so completely glued together that it was a work of some difficulty to get them open again. This case is recorded, with the names of the committee in the "Zoist," vol. iv., pp. 84—88; and I call the reader's attention to the *completeness* of the test here, and its demonstration of the reality of clairvoyance, as compared with the loose experiment and hasty jumping-to-a-conclusion in the case which Dr. Carpenter thinks alone worthy of record.

Dr. Carpenter next comes to the work of Professor Gregory ("Letters on Animal Magnetism,") and devotes several pages to assertions as to the professor's "credulity," the "reprehensible facility" with which he accepted Major Buckley's statements, the "entire absence of detail" as to "precautions against tricks," and his utter failure to find a clairvoyant to obtain Sir James Simpson's bank-note. "And yet," he says, referring especially to myself, "there are even now, men of high scientific distinction, who adduce Professor Gregory's testimony on this subject as unimpeachable!" Readers who have accompanied me so far, will at least hestitate to accept Dr. Carpenter's dictum on this point, till they have heard what can be said on the other side. To give full details would occupy far too much space, I must therefore refer my readers to Professor Gregory's book for some cases, and give merely a brief outline of others. At page 394 (Case 29) is given in detail a most remarkable test-case, in which Professor Gregory sent some handwriting from Edinburgh to Dr. Haddock's clairvoyant at Bolton; who gave in return a minute description of the writer, her appearance, dress, house, illness, medical treatment, &c. At page 401 another test of the same kind is described. At page 403 a number of such cases are summarised, and one very completely given in detail. At page 423 is an account of a clairvoyant boy at the house of Dr. Schmitz, Rector of the High School at Edinburgh. This boy described Professor Gregory's house accurately, and the persons at that time in the dining-room (afterwards ascertained to be correct). As a further test Dr. Schmitz was asked to go into another room with his son and do anything he liked. The boy then described their motions, their jumping about, the son going out and coming in again, and the doctor beating his son with a roll of paper. When Dr. Schmitz returned, Professor Gregory repeated all the boy had said, which the doctor, much astonished,

* This is a constant feature of the true mesmeric trance, but "Jack's" accusers seem to have known nothing about it.

declared to be correct in every particular. At page 445 (Case 42(is an account of another clairvoyant, a mechanic, who described Professor Gregory's house in detail, and saw a lady sitting in a particular chair in the drawing-room reading a new book. On returning home the professor found that Mrs. Gregory had, at the time been sitting in that particular chair, which she hardly ever was accustomed to use, and was reading a new book which had been sent to her just before, but of which the Professor knew nothing. At page 405 is a most remarkable case of the recovery of a stolen watch, and detection of the thief in London by Dr. Haddock's clairvoyant at Bolton. The letters all passed through Sir Walter C. Trevelyan, who showed them to Professor Gregory. At page 407 are the particulars of the extraordinary discovery of the locality of travellers by means of their handwriting only, sent from the Royal Geographical Society to Sir C. Trevelyan in Edinburgh, and by him to Bolton, he himself not knowing either the names of the travellers, or where they were. Many more cases might be referred to, but these are sufficient to show that there is not that "total absence of detail," and of "precautions," in Professor Gregory's experiments, which is Dr. Carpenter's reason for entirely ignoring them. In addition to this we have the account of Dr. J. Haddock, a physician practising at Bolton, of the girl Emma, who for nearly two years was under his care, and residing in his house. Many of Professor Gregory's experiments, and those of Sir Walter Trevelyan, were made through this girl, and a full account of her wonderful clairvoyant powers is given by Dr. Haddock in the Appendix to his "Somnolysm and Psycheism." She could not read, and did not even know her letters. The discovery of the stolen cashbox, and identification of the entirely unsuspected thief, is given in full by Dr. Haddock, and is summarised in my "Miracles and Modern Science," p. 64. Again, Dr. Herbert Mayo gives unexceptionable personal testimony to clairvoyance at pages 167, 172, and 178 of his book on "Popular Superstitions."

Dr. Carpenter is very severe on Professor Gregory for his belief in Major Buckley's clairvoyants reading mottoes in nuts, &c., but Major Buckley was a man of fortune and good position, who exercised his remarkable powers as a magnetiser for the interest of it, and there is not the slightest grounds for suggesting his untrustworthiness. We have beside the confirmatory testimony of other persons, among them of Dr. Ashburner, who frequently took nuts purchased by himself, and had them correctly read by the clairvoyants before they were opened. ("Ashburner's Philosophy of Animal Magnetism," p. 304.) Dr. Carpenter also doubts Professor Gregory's common sense in believing that a sealed letter had been read unopened by a clairvoyant when it might have been opened and resealed; but he omits to say that the envelopes were expressly arranged to pre-

vent their being opened without detection, and that the Professor adds, "I have in my possession one of the envelopes thus read, which has since been opened, and I am convinced that the precautions taken precluded any other than lucid vision."*

Still more important, perhaps, is the testimony of many eminent physicians to the existence of these remarkable powers. Dr. Rostan, Parisian Professor of Medicine, in his article "Magnetisme," in the "Dictionnaire de Medecine," says (as quoted by Dr. Lee), "There are few things better demonstrated than clairvoyance. I placed my watch at a distance of three or four inches from the occiput of the sonnambulist, and asked her if she saw anything. 'Certainly,' she replied, 'it is a watch; ten minutes to eight.' M. Ferrus repeated the experiment with the same successful result. He turned the hands of his watch several times, and we presented it to her *without looking at it;* she was not once mistaken." The Commissioners of the Royal Académie de Medecine applied the excellent test of holding a finger on each eye-lid, when the clairvoyant still read the title of a book, and distinguished cards. (Quoted in Dr. Lee's "Animal Magnetism," p. 22.) Dr. Esdaile had a patient at Calcutta who could hear and see through the stomach. This was tested by himself with a watch, as in the French case quoted above. ("Zoist," vol. viii., p. 220.) Dr. Teste's account of the clairvoyance of Madame Hortense is very suggestive. She sometimes read with ease when completely bandaged, and when a paper was held between her eyes and the object; at other times she could see nothing, and the smallest fatigue or excitement caused this difference. This excessive delicacy of the conditions for successful clairvoyance render all public exhibitions unsatisfactory; and Professor Gregory "protests against the notion that it is to be judged by the rough experiments of the public platform, or by such tests as can be publickly applied." For the same reason direct money tests are always objected to by experienced magnetisers, the excitement produced by the knowledge of the stake or the importance of the particular test impairing or destroying the lucidity. This is the reason why gentlemen and physicians like Professor Gregory, Major Buckley, and Dr. Haddock, who have had the command of clairvoyants, have not attempted to gain the bank-notes which have at various times been offered. Dr. Carpenter was very irate because I suggested at Glasgow—not as he seems to have understood that there *was* no note in Sir James Simpson's envelope—but that the clairvoyants themselves, if they heard of it, might very

* Dr. Carpenter says that "the unsealing of letters and the re-sealing them so as to conceal their having been opened" are practised in Continental post-offices. No doubt this can be done with an ordinary letter, but it is no less certain that there are many ways of securing a letter which absolutely preclude its being done undetected, and Dr. Carpenter omits to state that such precautions are here expressly mentioned by Professor Gregory as having been used in these experiments.

well be excused if they thought it was a trick to impose upon them. I find now that in the other case quoted by Dr. Carpenter, the note for £100 publicly stated to have been enclosed by Sir Philip Crampton in a letter, and placed in a bank in Dublin, to become the property of any clairvoyant who should read the *whole of it*—this was actually the case. After six months the letter was opened, and the manager of the bank certified that it contained no note at all, but a blank cheque! The correspondence on the subject is published in the "Zoist," vol. x., p. 35. Dr. Carpenter's indignation was therefore misplaced; for, as a medical knight in Ireland did actually play such a trick, the mere supposition on my part, that ignorant clairvoyants might think that a medical knight in Scotland was capable of doing the same, was not a very outrageous one.

We now come to the last part of Dr. Carpenter's lecture—Table-Turning and Spiritualism, and here there is hardly any attempt to deal with the evidence. Instead of this we have irrelevant matters put prominently forward, backed up by sneers against believers, and false or unproved accusations against mediums. To begin with, the old amusement of table-turning of fifteen or twenty years ago, with Faraday's proof that it was often caused by unconscious muscular action, is again brought to the front. Table-tilting is asserted to be caused in the same way, and an "indicator" is suggested for proving this; and the whole matter is supposed to be settled because no one, so far as Dr. Carpenter is aware, "has ever ventured to affirm that he has thus demonstrated the *absence* of muscular pressure," and "until such demonstrations shall have been given, the tilting—like the turning—of tables may be unhesitatingly attributed to the unconscious muscular action of the operators." We suppose Dr. Carpenter will shield himself by the "thus" in the above sentence, though he knows very well that a far more complete demonstration of the absence of muscular pressure than any indicator could afford has been repeatedly given, by motion, both turning and tilting, of the table occurring *without any contact whatever*. Thus, in the Report of the Committee of the Dialectical Society, we have (p. 378), Experiment 13, nine members present, all stood quite clear of the table, and observers were placed under it to see that it was not touched, yet it repeatedly moved along the floor, often in the direction asked for. It also jerked up from the floor about an inch. This was repeated when all stood 2 feet from the table. Experiment 22. Six members present, the same thing occurred under varied conditions. Experiment 38 (p. 390). Eight members present, the conditions were most rigid; the chairs were all turned with their backs to the table at a foot distant from it; every member present knelt on his chair *with his hands behind his back*; there was abundance of light, yet, under these test-conditions, the table moved several times in various directions, visible to all present. Finally

the table was turned up and examined, and found to be an ordinary dining table with no machinery or apparatus of any kind connected with it. Similar movements without contact have been witnessed elsewhere and recorded by Serjeant Cox and by Mr. Crookes, as well as by many other persons; yet the man who comes before the public as the "historian" of this subject tells his audience and his readers that "he is not aware that anyone affirms that he has demonstrated the absence of muscular pressure"! How are we to reconcile this statement with Dr. Carpenter's references to each of the books, papers, or letters containing the facts above quoted or referred to? But we have evidence of a yet more conclusive character (from Dr. Carpenter's own point of view), because it is that of a medical man who has made a special study of abnormal mental phenomena. Dr. Lockhart Robertson, for many years an editor of the "Journal of Mental Science" and Superintendent of the Hayward's Heath Asylum, declares that his own heavy oak dining table was lifted up and moved about the room, and this not by any of the four persons present. Writing was also produced on blank paper which the medium "had not the slightest chance of touching" ("Dialectical Report," p. 248). Dr. Carpenter is always crying out for "sceptical experts," but when they come—in the persons of Robert Houdin and Dr. Lockhart Robertson, he takes very good care that, so far as he is concerned, the public shall not know of their existence. What, therefore, is the use of his asking me (in a note at p. 108) whether my table ever went up within its crinoline in the presence of a "sceptical expert"? The very fact that I *secretly* applied tests (see "Miracles and Modern Spiritualism," p. 134) shows that I was myself sceptical at this time, and several of my friends who witnessed the experiments were far more sceptical, but they were all satisfied of the completeness of the test. The reason why some sceptical men of science never witness these successful experiments is simply because they will not persevere. Neither Dr. Carpenter nor Professor Tyndall would come more than once to my house to see the medium through whom these phenomena occurred, or I feel sure they might, after two or three sittings, have witnessed similar phenomena themselves. This has rendered all that Dr. Carpenter has seen at odd times during so many years of little avail. He has had one, or at most two, sittings with a medium, and has taken the results, usually weak or negative, as proving imposture, and then has gone no more. Quite recently this has happened with Dr. Slade and Mrs. Kane; and yet this mode of enquiry is set up as against that of men who hold scores of sittings for months together with the same medium, and after guarding against every possibility of deception or delusion obtain results which seem to Dr. Carpenter incredible. Mr. Crookes had a long series of sittings with Miss Kate Fox (now Mrs. Jencken) in his own

house, and tested the phenomena in every way his ingenuity could devise. Dr. Carpenter was recently offered the same facilities with this lady and her sister, but as usual had only one sitting. Yet he thinks it fair and courteous to make direct accusations of imposture against both these ladies. He revives the absurd and utterly insufficient theory that the "raps" are produced by "a jerking or snapping action of particular tendons of either the ankles, knees, or toes." The utter childishness of this explanation is manifest to any one who has heard the sounds through any good medium. They vary from delicate tickings to noises like thumpings with the fist, slappings with the hand, and blows with a hammer. They are often heard loudly on the ceiling or on a carpetted floor, and heard as well as felt on the backs or seats of chairs quite out of reach of the medium. One of the sceptical committees in America tested the Misses Fox by placing them barefooted on pillows, when the "raps" were heard as distinctly as before on the floor and walls of the room. Mr. Crookes states that he has heard them on the floor, walls, &c., when Miss Fox was suspended in a swing from the ceiling, and has felt them on his own shoulder. He has also heard them on a sheet of paper suspended from one corner by a thread held between the medium's fingers. A similar experiment was tried successfully by the Dialectical Committee ("Report," p. 383). At a meeting of the same committee raps were heard on a book while in the pocket of a very sceptical member; the book was placed on the table, and raps again heard; it was then held by two members supported on ivory paper knives, when still raps were heard upon it ("Report," p. 386).

Again, there is the evidence of Professor Barrett, an experienced physicist, who entered on this enquiry a complete sceptic. He tells us that he examined the raps or knockings occurring in the presence of a child ten years of age—that in full sunlight, when every precaution to prevent deception had been taken—still the raps would occur in different parts of the room, entirely out of reach of the child, whose hands and feet were sometimes closely watched, at other times held. The phenomena have been tested in every way that the ingenuity of sceptical friends could devise; and as Professor Barrett is well acquainted with Dr. Carpenter's writings on the subject and the explanations he gives, we have here another proof of the utter worthlessness of these explanations in presence of the facts themselves.

The Honourable R. D. Owen has heard, in the presence of Miss Fox, blows as if made by a strong man using a heavy bludgeon with all his force, blows such as would have killed a man or broken an ordinary table to pieces; while on another occasion the sounds resembled what would be produced by a falling cannon-ball, and shook the house ("Debateable Land," p. 275); and Dr. Carpenter would really have us believe that all these wonderfully varied sounds under all these test conditions are produced by "snapping tendons."

But what is evidently thought to be the most crushing blow is the declaration of Mrs. Culver given at length in the Appendix. This person was a connection of the Fox family, and she declared that the Misses Fox told her how it was all done, and asked her to assist them in deceiving the visitors; two gentlemen certify to the character of Mrs. Culver. The answer to this slander is to be found in Capron's "Modern Spiritualism," p. 423. Mr. Capron was an intimate friend of the Fox family, and Catherine Fox was staying with him at Auburn, while her sisters were at Rochester being examined and tested by the committee. Yet Mrs. Culver says it was Catherine who told her "that when her feet were held by the Rochester Committee the Dutch servant-girl rapped with her knuckles under the floor from the cellar." Here is falsehood with circumstance; for, first, Catherine was not there at all; secondly, the Committee never met at the Fox's house, but in various public rooms at Rochester; thirdly, the Fox family had no "Dutch servant-girl" at any time, and at that time no servant-girl at all. The gentlemen who so kindly signed Mrs. Culver's certificate of character did not live in the same town, and had no personal knowledge of her; and, lastly, I am informed that Mrs. Culver has since retracted the whole statement, and avowed it to be pure invention (see Mrs. Jencken's letter to "Athenæum," June 9, 1877). It is to be remarked, too, that there are several important mistakes in Dr. Carpenter's account. He says the "deposition" of Mrs. Culver was made not more than *six* years ago, whereas it was really *twenty-six* years ago; and he says it was a "deposition before the magistrates of the town in which she resided," by which, of course, his readers will understand that it was on oath, whereas it was a mere statement before two witnesses, who, without adequate knowledge, certified to her respectability!*

* Since the MS. of this article left my hands, I have seen Dr. Carpenter's letter in the "Athenæum" of June 16th, withdrawing the charges founded on the declaration of Mrs. Culver, which, it seems, Dr. Carpenter obtained from no less an authority than Mr. Maskelyne! the great conjurer and would-be "exposer" of spiritualism. He still, however, maintains the validity of the explanation of the "raps" by Professor Flint and his coadjutors, who are said to have proved that persons who have "trained themselves to the trick," can produce an "*exact imitation*" of these sounds. This "exact imitation" is just what has never been proved, and the fact that a "training" is admitted to be required, does not explain the sudden occurrence of these sounds as soon as the Fox family removed temporarily to the house at Hydesville. If Dr. Carpenter would refer to better and earlier authorities than Mr. Maskelyne and M. Louis Figuier, he would learn several matters of importance. He would find that Professors Flint, Lee, and Coventry, after one hasty visit to the mediums, published their explanation of the "raps" in a letter to the "Buffalo Commercial Advertiser," dated February 17th, 1851, *before* making the investigation on the strength of which they issued their subsequent report, which, therefore, loses much of its value since it interprets all the phenomena in accordance with a theory to which the reporters were already publicly committed. On this scanty evidence we are asked to believe that two girls, one of them only nine years old, set up an imposture which for a long time brought them nothing but insult and abuse, subjected their father to public rebuke from

This is an example of the reprehensible eagerness with which Dr. Carpenter accepts and retails whatever falsehoods may be circulated against mediums; and it will be well to consider here two other unfounded charges which, not for the first time, he brings forward and helps to perpetuate. He tells us that "the 'Katie King' imposture, which had deluded some of the leading spiritualists in this country, as well as in the United States, was publicly exposed." This alleged exposure was very similar to that of Mrs. Culver's, but more precise and given on oath—but the oath was under a false name. A woman whose name was subsequently discovered to be Eliza White declared that she had herself personated the spirit-form at several stated séances given by the two mediums Mr. and Mrs. Holmes, she having been engaged by them for the purpose; and she described a false panel made in the back of the cabinet by which she entered at the proper time from a bedroom in the rear. But Colonel Olcott, a gentleman connected with the New York daily press, has proved that many of the particulars about herself and the Holmes' stated in Mrs. White's sworn declaration are false, and that she is therefore perjured. He has also proved that her former character is bad; that the photograph taken of "Katie King," and which she says was taken from her, does not the least resemble her; that the cabinet used had no such moveable panel as she alleged; that the Holmes' manifestations went on just the same on many occasions when she was proved to be elsewhere; that she herself confessed she was offered a thousand dollars if she would expose the Holmes'; and, lastly, that in Colonel Olcott's own rooms, under the most rigid test conditions, and with Mrs. Holmes only as a medium, the very same figure appeared that was said to require the personation of Mrs. White.

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his minister, and made their mother seriously ill; and that they have continuously maintained the same for nearly thirty years, and in all this long period have never once been actually detected. But there are facts in the early history of these phenomena which *demonstrate* the falsehood of this supposition, but which Dr. Carpenter, as usual, does not know, or, if he knows does not make public. These facts are, firstly, that two previous inhabitants of the House at Hydesville testified to having heard similar noises in it; and, secondly, that on the night of March 31st, 1848, Mrs. Fox and the children *left the house*, Mr. Fox only remaining, and that during all night and the following night, in presence of a continual influx of neighbours *the "raps" continued exactly the same as when the two girls were present.* This crucial fact is to be found in all the early records, and it is surprising that it can have escaped Dr. Carpenter, since it is given in so popular a book as Mr. R. Dale Owen's "Footfalls on the Boundary of Another World" (p. 209). Mr. Owen visited the spot, and obtained a copy of the depositions of twenty-one of the neighbours, which was drawn up and published a few weeks after the events. This undisputed fact, taken in connection with the great variety of sounds—varying from taps, as with a knitting-needle, to blows as with a cannon-ball or sledge-hammer—and the conditions under which they occur—as tested by Mr. Crookes and the Dialectical Committee, completely and finally dispose of the "joint and tendon" theory as applicable to the ascertained facts. What, therefore, can be the use of continually trying to galvanise into life this thoroughly dead horse, along with its equally dead brother the table-turning "indicator"?

The full details are given in Colonel Olcott's "People from the Other World," pp. 425—478.

Another alleged exposure is introduced in the following terms:—"I could tell you the particulars, in my possession, of the detection of the imposture practised by one of the most noteworthy of these lady mediums in the distribution of flowers . . . these flowers having really been previously collected in a basin upstairs and watered out of a decanter standing by—as was proved by the fact that an inquisitive sceptic having furtively introduced into the water of the decanter a small quantity of ferrocyanide of potassium, its presence in the 'dew' of the flowers was afterwards recognised by the appropriate chemical test (a per-salt of iron) which brought out prussian blue."

In his article on the "Fallacies of Testimony," in the "Contemporary Review" of January, 1876, where Dr. Carpenter first gave an account of this alleged exposure, it is stated that "a basin-full of these flowers (hollyhocks) was found in a garret with a decanter of water beside it," that the ferrocyanide was mixed with this water, and that all this was not hearsay, but a statement in writing in the hand of the "inquisitive sceptic" himself. It turns out, however, that this part of the statement was wholly untrue, as we know on the authority of a letter written by the lady of the house, and afterwards published, and Dr. Carpenter now seems to have found this out himself; but instead of withdrawing it wholly (as in common fairness he ought to have done), he still retains it ingeniously modified into an *inference*, but so worded as to look like the statement of a *fact*;—"these flowers having *really* been previously collected in a basin," &c.,—"as was proved"—not by finding them, but by the chemical test! What an extraordinary notion Dr. Carpenter must have of what is "really" proof. Let us, however, look a little further into this matter, of which more is known than Dr. Carpenter adduces, or than he thinks advisable to make public. Dr. Carpenter's informant was a member of the family in whose house the medium was staying as a guest. He had therefore full knowledge of the premises and command over the servants, and could very easily have ascertained such facts as the bringing of a large bunch of hollyhocks, asters, laurels, and other shrubs and flowers into one of the visitors' bedrooms, and whether they disappeared from the room when the lady medium left it previous to the séance. This would have been direct evidence, and easily attainable by one of the family, but none such is forthcoming; instead of it we have the altogether inconclusive though scientific-looking chemical test. For it is evident that the flowers which appear must be brought from somewhere, and may naturally be brought from the shortest distance. If there are flowers in the house, these may be brought—as a baked apple was actually brought when an apple was asked for, according to one of the reports of this very séance;—and if a sceptic chooses to put chemicals with such

flowers or baked apples beforehand, these chemicals may be detected when the flowers or apples are examined. The wonder of such séances does not at all lie in where the flowers are brought from, but in the precautions used. The medium's hands, for instance, are always held (as they were in this instance) yet when thus held the flowers drop on to the table, and even particular flowers and fruits drop close to the persons who ask for them. This is the real fact to be explained when, as in this case, it happens in a private house; and the alleged chemical test has no bearing on this. But here the test itself is open to the gravest suspicion. The person who says he applied it had struck a light in the middle of the séance and discovered nothing. He was then, in consequence of some offensive remarks, asked to leave the room or the séance could not go on; and subsequently high words passed between him and the medium. He is therefore not an unbiassed witness, and to support a charge of this kind we require independent testimony that the chemical in question was not applied to the flowers *after* they appeared at the séance. This is the more necessary as we have now before us the statement in writing by another resident in the house, that some of the flowers were sent to a medical man in the town, and that no trace of ferrocyanide of potassium could be detected. The accuracy of the supposed tests is also rendered very doubtful by another fact. In a published account of the affair in the "Bath and Cheltenham Gazette," endorsed by Dr. Carpenter's informant (in a letter now before me) as being by a friend of his and substantially correct, it is stated that the "same authority" who is said to have "demonstrated the presence of potassium ferrocyanide" on the flowers also examined some sand which fell on the table at the same sitting, and found it to contain salt, and therefore to be sea-sand, and to agree microscopically with the sand from a sea-beach near which the medium had been staying a few days before. This reads very like truth, and looks very suspicious, but it happens that another gentleman who was present at the séance in question took away with him some of the sand for the purpose of subjecting it to microscopic examination; and from that gentleman—Mr. J. Traill Taylor, Editor of the "British Journal of Photography" and an occasional contributor to other scientific journals—I have received the following note on the subject:—"I remember the séance to which you have alluded, and which was held on the evening of August 23, 1874, during the Belfast Meeting of the British Association, which I was attending. At that time, among other bye-pursuits, I was engaged in the microscopical examination of sand of various kinds, and I omitted no opportunity of procuring samples. During my visit to Ireland I obtained specimens from the sea-coast of Counties Down and Armagh, as well as from the shores of Lough Neagh. When the shower of sand fell upon the table during the séance I appropriated a quantity of it for subsequent exami-

nation. The most careful inspection under the microscope satisfied me that it was absolutely identical with some that had been procured from the Antrim coast of Lough Neagh, while it differed in certain respects from that obtained at the sea coast. Having subsequently seen a communication on this subject in the "English Mechanic" (by a writer who, I believe, had not been present at the séance), the purport of which was that the séance sand was similar to some obtained from a part of the sea-coast where the medium had been recently residing, I again subjected these various sands to microscopical examination, only to be confirmed in my previous conclusion. I followed this by a chemical test as follows:—I washed each sample of sand in a test-tube with distilled water, to which I then added a solution of nitrate of silver. A precipitate of chloride of silver was obtained from all the samples of sea-sand, but no precipitate was formed by that which came from Lough Neagh nor by that obtained at the séance, which last, under this chemical test behaved in a manner precisely similar to the Lough Neagh sample. I recollect that the result of this test was my feeling sure that the writer to whom I have alluded had not had the same data as those in my possession for arriving at a conclusion. In about a year after that time I threw away over a dozen different samples of sand, including those to which I have referred, as I required for another purpose the boxes in which they had been kept."

This clear and precise statement demonstrates the untrustworthiness of the authority on whom Dr. Carpenter relies, even if it does not indicate his disposition to manufacture evidence against the medium in question. At all events, with the more complete account of the whole episode now before them, our readers will, we are sure, admit that the evidence is by no means free from suspicion, and is quite insufficient to justify its being used to support a public charge of deliberate imposture. It also affords another example of how Dr. Carpenter jumps at explanations which are totally inapplicable to the facts in other cases, as, for example, to the production of flowers and ferns in my own room, as narrated in my "Miracles and Modern Spiritualism," page 164, and to that in the house of Mr. T. Adolphus Trollope, as given in the "Dialectical Report," pp. 277 and 372, in which case the medium had been carefully searched by Mrs. Trollope before the séance began.

We have now only to notice the extraordinary Appendix of *pieces justificatives*, which, strange to say, prove nothing, and have hardly any bearing on the main questions at issue. We have, for instance, six pages of extracts on early magic, the flagellants, and the dancing mania; followed by four pages about Mesmer; then an account of Mr. Lewis's experiments before the Medical School, Aberdeen, which failed; then eight pages on the effects of *suggestion* on hypnotised patients—effects thoroughly known to every operator, but having no bearing on the case of

persons never hypnotised or mesmerised, and to whom *no suggestion* was made; after this comes ten pages on the planchette, on which no one relies without collateral evidence; and then an account of some foolish clergymen, who thought they had direct proof of Satanic agency; then comes Mrs. Culver's statement (called a "deposition before magistrates" in the text), to which we have already referred; then my own letter to the "Spectator" about Mr. G. H. Lewes's supposed proof of the imposture of Mrs. Hayden; then the oft-told story of Dr. Carpenter's interviews with Foster, from the "Quarterly Review" article; then more of Mr. Braid's "suggestion and expectancy" experiments, —and that is all! Not one solitary piece of careful investigation or unimpeachable evidence in these forty-two pages of what are announced as *pieces justificatives!*

Let us now summarise briefly the results of our examination of Dr. Carpenter's book. We have given a few examples of how he has misrepresented the opinions of those opposed to his theories. Although he professes to treat the subject historically, we have shown how every particle of evidence is ignored which is too powerful to be explained away. As examples of this we have referred, in more or less detail, to the denial by high authorities of the reality of painless surgical operation during the mesmeric sleep; to the "Report of the Royal Académie de Medécine," supporting the reality of clairvoyance and the other higher phenomena of mesmerism; to experiments on clairvoyance, before French medical sceptics; to the evidence of educated and scientific men in Vienna as to the truth of Reichenbach's observations: to the personal evidence of Robert Houdin, Professor Gregory, Dr. Mayo, Dr. Haddock, Dr. Lee, Dr. Ashburner, Dr. Rostan, Dr. Teste, and Dr. Esdaile, as to tests demonstrating the reality of clairvoyance; to the evidence of the Dialectical Committee, of Dr. Lockhart Robertson, Serjeant Cox, Mr. Crookes, and myself, as to motion of solid bodies demonstrably not caused by muscular action; to the evidence of the Dialectical Committee, of the Hon. Robert Dale Owen, Mr. Crookes, and Professor Barrett, as to raps demonstrably not caused by the muscles or tendons of the medium; to the evidence of Mr. T. A. Trollope and myself as to the production of flowers, demonstrably not brought by the medium,— all of which evidence, and everything analogous to it, is totally ignored by Dr. Carpenter. Again, this work, professing to be "scientific," and therefore accurate as to facts and precise as to references, has been shown to be full of misstatements and misrepresentations. As examples we have—the statement that there is no evidence of the mesmeriser's power to act on a patient unconscious of his wish to do so, whereas I have shown that there is good medical evidence of this power; that Reichenbach did not submit his subjects to tests, whereas I have quoted many admirable tests, as well as the independent test-observa-

tions of Dr. Charpignon; that Rutter's magnenometer never acted when the operator did not know the substance influencing it, whereas Mr. Rutter states clearly and positively that it did; that the Royal Academy of Medicine *first* investigated clairvoyance in 1837 and declared it not proved, whereas they first investigated it in 1825, and reported *favourably;* that Professor Gregory was credulous, and took no precautions against imposture, which I have shown to be not the fact. Again we have numerous errors and misstatements (always against the mediums) in the accounts of the Misses Fox and Mrs. Culver, of the alleged "Katie King" exposure, and of the flower-séance chemically exposed. And, lastly, we have the statement, repeated under many forms, that when adequate investigation has taken place, and especially when "trained experts" have been employed, trick or imposture has *always* been discovered. But this I have shown to be the grossest of all misstatements. Surely medical men are "trained experts," and we have nine members of the Royal Academy of Medicine investigating for five years, and a large number of French and English medical men devoting years of enquiry to this subject, and deciding that it is *not* imposture. Are not eminent physicists trained experts, so far ar least as the purely physical phenomena are concerned? But we have Prof. Hare, Prof. Gregory, and Mr. Crookes, who all devoted years of careful investigation to the subject; Prof. Barrett, who has come to it with a fresh and sceptical mind, stored with all the warnings that Dr. Carpenter can give him, and yet declares it to be reality, and neither imposture nor delusion; while another recent convert from extreme scepticism on this subject is Dr. Carter Blake, Lecturer on Comparative Anatomy at Westminster Hospital, who last year wrote me that after months of careful examination he was satisfied that the phenomena called "Spiritual" are thoroughly genuine and worthy of scientific examination,—that he has arrived at this conclusion very slowly, and, referring to his recent investigations, he says—"Every experiment performed has been under the most rigorous test conditions, and the dishonest element which some professional mediums have shown has been rigorously eliminated. Yet, again, professional conjurors are surely "trained experts," and Dr. Carpenter has himself often referred to them as such, but the moment they go against him he ignores them. I have adduced, for the second time, the remarkable evidence of Robert Houdin to the reality of the clairvoyance of Alexis; Mr. T. A. Trollope informs us that another celebrated conjuror, Bosco, "utterly scouted the idea of the possibility of such phenomena as I saw produced by Mr. Home being performed by any of the resources of his art;" and lastly, at Glasgow, last year, Lord Rayleigh informed us that he took with him a professional conjuror to Dr. Slade's, that the phenomena happened with considerable perfection, while "the

conjuror could not form the remotest idea as to how the effects were produced."

We have now concluded what has been a painful task; but in the interests of truth it was necessary to show how completely untrustworthy is the self-appointed guide that the public so blindly follow. By ample references I have afforded to such of my readers as may be so inclined the means of testing the correctness of my charges against Dr. Carpenter; and if they do so they will, I feel convinced, not only lose all faith in his explanations of these phenomena, but will also find how completely ignorant of this, as of most scientific subjects, are those writers in our influential literary press who have, almost without exception, praised this book as a fair and complete exposition of the subject on which it treats.

It also seems to me that an important question of literary morality is here involved. While maintaining as strongly as anyone that new or disputed theories should be subjected to the fullest and severest criticism, I yet hold that this should not involve either misrepresentation or what has been termed the "conspiracy of silence." It is, at the best, hard enough for new truths to make their way against the opposing forces of prepossession and indifference; and bearing this in mind, I would ask whether it is in the interests of human progress and in accordance with right principles, that those who have the ear of the public should put forth, under the guise of impartial history, a thoroughly one-sided and erroneous account of a disputed question. It may be said that errors and misstatements can be exposed, and will only injure the author of them; but unfortunately this is not so. The popular view of a subject like this is sure of a wide circulation, and writers in the daily and weekly papers increase its publicity, whereas few read the answer, and the press decline or refuse to make it known.*

* A striking proof of this statement has been quite recently furnished us. The letter given below was sent by Dr. Slade to Professor E. R. Lankester. It would seem to exhibit, in a high degree, the characteristics of truth, fairness, and charity. No answer was received. The press, moreover, refused to publish it, and the daily press, one and all, *refused to insert it even as an advertisement!*

"Professor E. R. Lankester.

"Dear Sir,—Dr. Slade having in some measure recovered from his very severe illness, and his engagement to St. Petersburg having been postponed (by desire of his friends there) till the autumn, desires me to make the following offer:—

"He is willing to return to London for the express and sole purpose of satisfying you that the slate-writing occurring in his presence is in no way produced by any trickery of his. For this purpose he will come to your house unaccompanied by any one, and will sit with you at your own table, using your own slate and pencil; or, if you prefer to come to his room it will suit him as well.

"In the event of any arrangement being agreed upon, Slade would prefer that the matter should be kept strictly private.

"As he never can guarantee results, you shall give him as many as six trials, and more if it shall be deemed advisable.

"And you shall be put to no charge or expense whatever.

As the very existence of the press depends on popularity this is inevitable, but it none the less throws a great reponsibility on those who possess this popularity if they mislead public opinion by inaccuracy or suppression of facts.

In his article on "Fallacies of Testimony" Dr. Carpenter, quoting Schiller, says, that the "real philosopher" is distinguished from the "trader in knowledge" by his always loving truth better than his system. If our readers will carefully weigh the facts now laid before them, they will be able to decide how far Dr. Carpenter himself belongs to the first or to the second of these categories.

ALFRED R. WALLACE.

Text-Book of Structural and Physiological Botany. By OTTO W. THOME. Translated and Edited by ALFRED W. BENNETT, F.L.S. London: Longmans and Co.

WE have here a translation of a German work which has found great approbation in its own country, and will probably experience an equally favourable reception in England. It embraces the whole range of elementary botany, and will prove a safe and convenient guide for the student in the earlier part of his career. The Editor, however, very judiciously reminds his readers that in Natural Science "the greater and the most useful part of the student's knowledge must always be acquired in the field, or with the dissecting knife in hand," the use of text-books being merely to put him in the right track for personal research, and to save him from the necessity of re-discovering what others have already

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"You on your part shall undertake that during the period of the sittings, and for one week afterwards, you will neither take, nor cause to be taken, nor countenance legal proceedings against him or me.

"That if in the end you are satisfied that the slate-writing is produced otherwise than by trickery, you shall abstain altogether from further proceedings against us, and suffer us to remain in England, if we choose to do so, unmolested by you.

"If, on the other hand, you are not satisfied, you shall be at liberty to proceed against us, after the expiration of one week from the conclusion of the six or more experiments, if we are still in England. You will observe that Slade is willing to go to you without witnesses of his own, and to trust entirely to your honour and good faith.

"Conscious of his own innocence, he has no malice against you for the past. He believes that you were very naturally deceived by appearances, which, to one who had not previously verified the phenomena under more satisfactory conditions, may well have seemed suspicious.

"Should we not hear from you within ten days from this date, Slade will conclude that you have declined his offer.

"I have the honour to be, sir, your obedient servant,

"J. SIMMONS."

37, Spui-straat, The Hague, May 7th 1877.

observed before him. The real educational value of Natural History, the development of the power of observation, will utterly escape those whose studies are confined to books.

The successive chapters of the work are devoted to a consideration of the cell as an individual; the cell as a member of a group of similar cells; the construction of the plant out of cells; the external form of plants; the life of the plant; special morphology and classification; the changes in the vegetation of the globe during past geological epochs; and botanical geography. The last division is illustrated with a map, showing the twenty-four regions into which the earth is divided by Griesbach in his "Végetation der Erde"—a classification which Mr. Bennett thinks "too unqualified," both with regard to the boundaries between the regions and the characters which distinguish them from one another. It is interesting to compare these regions with the geographical divisions of the animal world, as laid down by Mr. Wallace. We must own to a little surprise at finding Madagascar classed as an "Oceanic Island," along with the Azores, Madeiras, and Canaries.

The chapter on vegetable palæontology gives an accurate but necessarily very brief account of the floræ of bygone ages.

In the section on the "Life of the Plant," the influence of temperature, light, &c., upon vegetation is carefully described.

The work is throughout abundantly illustrated, and will, we hope, prove useful to those real students who seek not to "pass," but to know.

The Geology of England and Wales. A Concise Account of the Lithological Characters, Leading Fossils, and Economic Products of the Rocks; with Notes on the Physical Features of the Country. By Horace B. Woodward, F.G.S., of the Geological Survey of England and Wales. London: Longmans and Co.

Many as have been the geological works recently put forth in England, there was still, we believe, room for a condensed work of reference on the geology of our own country that should be fully on a level with the present state of the science. This want Mr. Woodward has supplied in what we must pronounce a very satisfactory manner. His treatise is well arranged, comprehensive, accurate, and concise. All unnecessary verbiage has been carefully avoided, so that the student is not placed under the necessity of seeking out the facts he wants amidst a dreary waste of padding. Of speculation there is little. The author declares himself to be what is technically called a "uniformitarian," but he judiciously adds that "in concluding that the physical forces have been the same throughout geological time" we must guard

against supposing their action to have been "always of similar intensity to that of which we have definite proof in the present." He also lays down a limitation which is sometimes conveniently overlooked by cavillers at geology and geologists. "The special province of the geologist," he reminds us, "is only to deal with the Earth after it was in a fit state to receive and support life, when the proportion of land to water was probably much as it is now, and the climate and physical conditions, though ever varying over the same area during the different geological ages, were subject to the same laws and attended by analogous phenomena. . . . To go back to the very earliest history of the Earth, when it was part of a nebulous mass, would be to trespass upon the region of the astronomer, and when we consider its latest history we come upon questions which must be answered by the geographer and the archæologist."

The author evidently accepts, in its general principles, the doctrine of Organic Evolution. He quotes, at any rate without formal disapproval, the opinion of Prof. Huxley, that "the less regard palæontologists pay to the deposit from which fossils are obtained so much the better, for not unfrequently has a new name been given to a known fossil because it has been found in strata where it was previously unknown." The same eminent author even longs for "a new race of palæontologists, utterly ignorant of geology"—a pious wish in which we find ourselves unable to join.

In treating of the economic bearings of geology, Mr. Woodward remarks that "the relation between health and geology is also a point which has in recent years received a good deal of attention, and maps have been published and memoirs written to show the relation between certain forms of disease and geological structure—even between geology and lunacy! It is well known, indeed, that a gravelly, sandy, or chalky soil is more healthy than a clay foundation, because the former are pervious to water and the latter is impervious. On the former there is less consumption than on the latter, as Mr. Whittaker and Dr. Buchanan have clearly demonstrated: the artificial removal of subsoil water has, however, done much to equalise the conditions. Again, the water-supply is a most important subject, for in some small country villages and towns the inhabitants suffer very much from its impurity. Situated, perhaps, on elevated ground, with a good porous soil, they yet suffer because of the disgraceful state of the drainage, the wells being shallow, and the sewage and even the churchyards draining into them. The cause of teetotalism will not find many admirers when it is often the women and children who suffer most from drinking impure water, while the men who take their beer are less subject to disease."

We fear that the sentence last quoted will bring down upon Mr. Woodward the gravest denunciations of the "temperance

party." But we cannot help remarking that it is in districts having a porous subsoil where polluted waters, and other evils arising from sanitary neglect, are most rampant. A stiff compact clay opposes an impassable barrier to the diffusion of cesspool and churchyard drainage. But over chalk and gravel, the cesspool and the well, though separated by an interval of perhaps a dozen yards, are practically identical. In a small town in Kent we even found that the sewage, after removal of the solids by means of subsidence-pits, was allowed to soak down and disappear in the permeable subsoil, which must thus become irredeemably saturated with putrescent matter, and must constantly give off noxious gases and vapours.

As regards the antiquity of the human species, the author admits that it appeared on the earth's stage unquestionably at a date very much earlier than our forefathers imagined. With Mr. Boyd Dawkins, he holds that man was co-existent, in this country and in Western Europe, with the lion, the hairy elephant, and the woolly rhinoceros, and that if his existence can be traced back to or even beyond the Glacial epoch, extending from 240,000 down to 80,000 years ago, a still higher antiquity must be assigned him.

The work is admirably illustrated, and is furnished with a good geological map of England and Wales, with a glossary of technical terms, a synopsis of the animal kingdom, a list of the principal works consulted, and a bibliography of the geology of the English counties.

We consider that Mr. Woodward's work merits almost unqualified commendation.

The Whitworth Measuring-Machine, including Descriptions of the Surface-Plates, Gauges, and other Measuring-Instruments, made by Sir Joseph Whitworth. By T. M. Goodeve, M.A., and C. P. B. Shelley, C.E. London: Longmans and Co. 1877.

We all know how very superior English machinery is to that which is manufactured by many other countries, but we do not always recognise the great share which Sir Joseph Whitworth has had in perfecting the machinery whereby our most accurate work is done. The authors of the work before us state at the outset that "the two principal surfaces of essential importance in the workshop may be distinguished as a 'true plane' and a 'true cylinder.'" The nearest approach to the former is a surface of clean mercury at rest. Sir Joseph Whitworth has devoted much time and thought to the production of perfectly plane surfaces and accurate methods of minute measurement, and his instru-

ments approach to accuracy as nearly as is possible. Messrs. Goodeve and Shelley have given an interesting account of the instruments, accompanied by good plates and woodcuts.

The Theory of Sound. By Lord Rayleigh, M.A., F.R.S., formerly Fellow of Trinity College, Cambridge. Vol. I. London: Macmillan and Co.

Every student of the mathematical theory of sound has felt the exceeding meagreness of the information on the subject within his reach. The ordinary text-books treat of Sound merely by way of an appendix to Dynamics and Hydro-dynamics, and consequently limit themselves to the most elementary parts of the theory, with scarcely a hint as to the existence of a mass of important recent investigations sufficiently numerous and valuable to require a separate treatment of the whole subject. The book before us is the first instalment of a work intended to remedy this deficiency, and certainly, so far as the range of this first volume extends, achieves a success.

After some valuable preliminary remarks on the ordinary theory of musical notes as due to vibrations, amongst which is to be found a remarkably clear *exposé* (p. 17) of the reasons for supposing the sensation of a simple tone to be due to a simple harmonic vibration, the author proceeds to describe various appliances for investigating the rapidity of the simple vibrations which may be present in any compound vibratory motion, illustrating at the same time the laws of superposition of small motions without interference, and showing very clearly the nature of the interference effect, in a simple case, of the superposition of motions which are so great that the squares and higher powers of the disturbance cannot be neglected—an effect which shows itself in the production of vibrations whose periods are the doubles, sum, and difference of the periods of the originals. The application of the method of generalised co-ordinates to the problem of small vibrations about an equilibrium position is then discussed, and the main part of the work consists of the application of the results to the detailed examination of the vibrations of strings, bars, membranes, and plates. This discussion contains all that is most important in the work of recent investigators, amongst whom Lord Rayleigh holds a foremost place.

The theory of sound is perhaps somewhat uninteresting as yet to the general mathematical reader, but it is full of interest to the investigator on account of the facility and delicacy of the acoustical tests which can be applied to verify theoretical deductions, and the author has taken care to supply this element of interest in this work by constant exhibition of the results of a

comparison between theory and observation. The result is a book which, when sound is applied as an instrument of discovery in molecular physics, will be found to be the most complete grammar we at present possess of the language in which the results of experiment will express themselves.

Elementary Text-Book of Physics. By G. D. Everett, M.A., D.C.L. London: Blackie and Son. 1877.

This work is intended for school classes, and passes in brief review the principal physical sciences. It is fairly illustrated, and contains many examples and questions to be worked out. It does not appear to us to possess any advantage over some of the very numerous text-books of physics which have been written with the same object during the last ten years.

Handbook of Natural Philosophy. Heat. By Dionysius Lardner, D.C.L. New Edition, edited by Benjamin Loewy, F.R.A.S. Lockwood and Co. 1877.

This is the familiar treatise on Heat of Dr. Lardner, enlarged so as to bring it in accordance with the most modern ideas on the subject. There are only a few new illustrations, but no less than 320 pages of new matter, among which will be found an interesting chapter on Dissociation and the Chemical Effects of Heat, and on the Dynamical Theory of Heat. Mr. Loewy has done his work of editing in a very satisfactory manner, and this work is still in many respects the most generally useful treatise on Heat which exists in our language.

Theoretical Naval Architecture: a Treatise on the Calculations involved in Naval Design. By S. J. P. Thearle. 2 vols. London: William Collins and Sons. 1877.

This work is divided into six parts, which respectively embrace —Calculations relating to (*a*) the forms and dimensions of ships; (*b*) the weights and centres of gravity of ships; (*c*) the strength of ships; (*d*) the propulsion of ships by sails; (*e*) the propulsion of ships by steam-engines; and (*f*) the steering of ships. In great ship-building cities like Glasgow the information herein contained will be eagerly read not only by master-shipwrights, but by the workmen themselves. A separate Atlas

contains sixty well-drawn plates, and a number of tables. The book is likely to become a standard work, and it supplies a want which has been felt.

Elements of Magnetism and Electricity. By JOHN ANGELL. London and Glasgow: William Collins and Sons. 1877.

THIS book forms one of Messrs. Collins's "Elementary Series" of science class books. Since it was first issued it has been very much improved by the addition of new matter, new illustrations, and the examination questions which have been set at South Kensington since 1867 for the "Elementary Stage." The book is very cheap, and is well illustrated; if it has a fault, it is that the author has attempted to crowd too much matter into a small space, and the explanations are sometimes less full than might be desired; but it is quite sufficient for, and suitable to, the students who present themselves for the elementary stage, and it forms a useful class book for schools.

The Winds and their Story of the World. Vis Inertiæ *in the Ocean.* By W. L. JORDAN, F.R.G.S. Hardwicke and Bogue. 1877.

IN the first of these works the author, in discussing the nature of the winds, endeavours to trace a connection between the vortices of Descartes and Newton's law of gravitation. In the second the results of recent oceanic exploration are considered in reference to the *vis inertiæ* of the ocean. The main object of the author seems to be to refute certain assertions of Prof. Huxley and Dr. Carpenter.

What is Vital Force? Or a Short and Comprehensive Sketch, including Vital Physics, Animal Morphology, and Epidemics, to which is added an Appendix upon Geology: Is the Detrital Theory of Geology Tenable? By RICHARD FAWCETT BATTYE. London: Trübner. 1877.

THIS curious book appears to have been written with a view of ventilating certain pet theories of the author. Human progress is so rapid, and scientific workers are so numerous, that it is as much as most people can do to keep *au courant* with the result of real research, upon which all truth must be founded. Hence we do not predict for Dr. Battye's book a very large number of readers, and we are afraid that we cannot class ourselves among them.

Aids to Chemistry, specially designed for Students preparing for Examinations. By C. E. Armand Semple, B.A. London: Bailliere, Tindall, and Cox. 1877.

This little book forms an useful abstract of the history of the non-metallic elements, and might serve as an example of good lecture-notes to those attending lectures on chemistry for the first time.

The Aquarium; its Inhabitants, Structure, and Management. By J. E. Taylor, Ph.D., F.L.S., &c. London: Hardwicke and Bogue.

The author of this book refers in his opening chapter to "a good deal of quibbling which has taken place respecting the word 'Aquarium,'" but he considers that it has now "gained its ground," and has "passed out of the regions of philology into that of common parlance." This is all very true as regards the present, but we fear Dr. Taylor has overlooked the probability of more serious discussions in the future. We can imagine in the year of grace 2877, if the world, our modern civilisation, and the English language, should last so long, some Dr. Dryasdust reading a paper on the primitive character of aquaria, and exciting the incredulity of the public by maintaining that they were originally destined to promote the study of natural history, and once contained fishes other than those facetiously characterised as "loose." It is but too probable that these establishments, whilst increasing in number and in popularity, are being developed as general places of amusement in which natural history will play a part about as important as did the poor halfpenny-worth of bread in Falstaff's tavern bill. Dr. Taylor, however, addresses himself to the lovers of the aquarium in its original sense, as a place where the habits and development of aquatic animals may be studied, and where valuable observations, economical as well as theoretical, may be and have been made.

The first two chapters of the book are devoted to a history of aquaria with a notice of such as have acquired especial reputation. Among these a prominent place belongs to the Naples Aquarium or zoological station founded by Dr. Dohrn, with the assistance of Mr. Lloyd and that of Professor Agassiz, at Penékese Island, near New York. These establishments are *bona fide* places for thorough scientific work, fitted up with dissecting rooms, physiological laboratories, &c., and are open to receive students and others desirous of conducting researches in marine zoology. It is scarcely needful to say that "rinking," promenade concerts, and theatricals form no part of the programme. Such truly scientific aquaria are much needed at such

places as Ceylon, Mauritius, Labuan, near Sydney, in the West Indies, &c., and if placed under suitable management could not fail to yield a rich harvest of valuable results. Are there no wealthy men at home or in the colonies able and willing to follow the example of that true "merchant prince" who defrayed the expense of the great Penékese Aquarium?

In the next chapter Dr. Taylor enters upon the "principles of the aquarium," which he recommends as a feature in private houses valuable as a source of comfort and enjoyment to invalids and sedentary persons, as a means of cherishing a love for natural history and as a means of moral education for children. This latter function it fulfils by neutralising that fondness for inflicting pain and death which is unfortunately so deeply rooted in the English nature. The author maintains that a fresh-water "aquarium properly constructed and peopled with proper inhabitants gives very little trouble indeed." He cautions beginners against the common error of overstocking their aquaria and of exposing them to an excessive amount of light, the result of which is the development of a green film of *Algæ* over the glass and over the water-plants. A well-balanced aquarium does not require a frequent change of water. In succeeding chapters an account is given of the amphibians, fishes, insects, and plants best suited for stocking a fresh-water tank, and an especial and interesting section is devoted to the "aquarium as a nursery for the microscope," or rather for microscopic objects.

The author then turns to the construction and management of salt water or marine aquaria, which present undoubtedly greater difficulties, and require to be on a larger scale, but which, on the other hand, offer a much wider scope.

The work must be pronounced to be clearly and ably written, well and abundantly illustrated, and will doubtless draw increased attention to aquaria, and thus render them more serviceable to science.

SCIENTIFIC NOTES.

The western states and territories of the American Union have suffered very seriously from the ravages of noxious insects, among which the locust, or hopper, as it is familiarly called, holds a bad pre-eminence. Under these circumstances it is very fortunate that the American Government and people do not, like so many nearer home, look upon entomology as a frivolous and useless pursuit, but have of late years given decided encouragement to its cultivation. A commission has been appointed to investigate and report on the best means of combatting the locust. The commissioners, Messrs. C. V. Riley, A. S. Packard, Jun., and C. Thomas, have all "made their mark," and the public may rest assured that their recommendations will be based upon an accurate knowledge of the habits of the destroyer. The United States Commissioners propose, as the means of destroying the eggs and newly hatched young, ploughing, with subsequent harrowing and rolling. For the first eight or ten days after hatching, and in the mornings and evenings subsequently, they are sluggish, huddle together, and may be easily driven into rows of burning straw, or into ditches about 2 feet wide by 2 feet deep, and with steep sides. Wider ditches permit their escape more readily, except the depth be increased likewise. Various kinds of drag-nets and bags, to be drawn by hand or horse-power, have also been used with great effect. With one of these, in Minnesota, from eight to twelve bushels of pupæ have been taken daily. The Commissioners finally insist strongly on the protection of all insectivorous birds—a very judicious recommendation.

The records of the "United States Geological and Geographical Survey of the Territories" always contain much valuable information. No. 4 of vol. ii. of the Bulletin comprises five important memoirs, namely, "Notes on the Geology of North-eastern New Mexico," by O. St. John; "Sexual, Individual, and Geographical Variation in Leucosticte Tephrocotis," by J. A. Allen; "Geographical Variation among North American Mammals, especially in respect to Size," by J. A. Allen; "Descriptions and Illustrations of Fossils from Vancouver's and Sucia Islands, and other North-western Localities," by F. B. Meek; and "Note on the New Genus Uintacrinus," by F. B. Meek. Mr. Allen after a careful examination of the series of skulls of the North American mammalia contained in the National Museum—a collection which he may well term "magnificent," amounting as it often does to 80 to 100 specimens of a single species—has been greatly struck with the different degrees of variability exhibited by representatives of species and genera even of the same family. In the wolves and foxes the variation in size with latitude amounts to 25 per cent of the average size of the species, while in other species of the *Feræ* it is almost *nil*. The common supposition, however, that the size of a species decreases with a decrease in the latitude cannot be admitted as a general law, since, in some forms, there is a well-marked increase as we proceed southward. He expresses the connection of size with geographical distribution in the following laws:—(1.) "The maximum physical development of the individual is attained where the conditions are most favourable to the life of the species." (2.) "The largest species of a group are found where the group to which they severally belong reaches its highest development, or where it has what may be termed its centre of distribution." (3.) "The most 'typical' or most generalised representatives of a group are found also near its centre of distribution, outlying forms being generally more or less 'aberrant' or specialised." The first portion of vol. iii. is a description of new genera and species of dipterous insects from the region west of the

Mississippi and especially from California, by C. R. Osten Sacken. The author expresses himself convinced that "the western fauna is essentially *one*, and that many of the characteristic forms of California sooner or later will turn up in Colorado." He has traced unexpected analogies and coincidences between the fauna of California and those of Europe, Chili, and even Australia, and, on the other hand, has found unforeseen differences from the fauna of the Atlantic States. His final summary of results is exceedingly instructive, and must be regarded as a most valuable contribution to animal geography. Contrary to his former belief he finds that the Rocky Mountains do not form a natural boundary for a distinct entomological fauna. The true boundary is an imaginary line, which some place about longitude 98° W., and others about 100°, and to the west of which agriculture becomes precarious without the aid of artificial irrigation, owing to the intense dryness of the summers. In this region the insects, like all other organisms, have to adapt themselves to the prevailing meteorological conditions. This accounts for the prevalence of the Heteromera among beetles. These insects have a remarkable power of resisting desiccation, owing to the toughness of their integuments, and being nocturnal they escape the action of the sun. Like the *Carabidæ* of the same region, also nocturnal, they have usually a black colour. The same dryness of soil and climate fosters the fossorial Hymenoptera, and the nests of these are again infested by *Melœdæ* among Coleoptera, and *Bombylidæ* among Diptera. Similar climatic conditions occur in the eastern hemisphere in the Mediterranean region and Central Asia, and there also the same forms of life prevail. The author, however, holds that the resemblance between the West American and the Mediteranean faunæ is not a relationship, but a mere analogy, due to similar climatic conditions. Of insects the same families prevail, but not necessarily the same genera. A totally different case is the resemblance between the Californian fauna and that of Northern and Central Europe. Here, instead of similarity, there is a striking difference in climate. That, therefore, European species should be found in California, Texas, and Colorado and yet be wanting in the Atlantic States, points to "some hidden genetic connection between the faunæ of Europe and of Western America." Thus the common magpie, unknown in the Eastern States, is not uncommon in California. Among insects the author furnishes many curious instances of this similarity. Sometimes genera are common to the two regions, sometimes species, and sometimes, again, Californian forms find their nearest representatives in Europe. Our English *Papilio Machaon* is almost identical with the *P. Zolicaon* of California. *Plusia gamma*, one of the most abundant European moths, is very common in California, but does not occur east of the Mississipi. The distribution of the dipterous genus *Apiocera* is most remarkable; hitherto it has been only found in Chili and Australia, but the author describes a species from California. Mr. P. R. Uhler's report on the insects collected by himself in 1875, and on the Hemiptera collected by Dr. Packard, Jun., is also important. He describes insect life as very abundant in Colorado. "On the open commons of the city of Denver," he writes, "I was delighted to see large patches of showy flowers, and to observe how certain insects of similar colours flew to and rested upon them. Very conspicuously was this the case with a delicate blue Lupin. Two species of the little blues, *Lycæna melissa* and *L. rapahæ*, settled upon these flowers, and when at rest were very difficult to recognise." The swarms of grasshoppers—one of the greatest pests of Western America—attracted the author's attention. He noticed that the females were much more numerous than the males—in the cases where he was able to count even to the extent of ten to one. Into the lists of insects captured or seen, and the detailed descriptions of the Hemiptera, we cannot of course enter. The same remark must apply to Dr. A. S. Packard's descriptions of Dr. Thorell's collection of spiders from the Colorado territory.

From the records of the "Geological Survey of India" we learn with regret the resignation of Dr. Oldham, owing to declining health. As Superintendent of the Geological Survey of India and President of the Asiatic Society of Bengal, he has "made his mark" and rendered the most important services to

science. In part 2 of vol. ix. of these Records Mr. Lydekker gives an interesting description of a cranium of *Stegodon Ganesa*, with notes on the sub-genus and on allied forms. From the number of species and genera found in the Siwaliks and other Indian strata he considers that India was the original home of the Proboscidians. *Elephas, Mastodon, Deinotherium, and Tapirus* are all found fossil in that country. The migration probably took plase from thence, where all the sub-genera had arisen long before the Siwalik times. "*Loxodon planifrons* or an unknown allied species might have travelled westwards and given rise to *Loxodon meridionalis* of the English 'Forest Beds,' and subsequently to the living *Loxodon africanus*. *Euclephas* may first have given rise to the Siwalik species, from which again sprang the Narbada species and the living *Euclephas indicus*, and, on the other hand, to another branch which travelled over Asiatic Russia and thence to Europe, producing the Mammoth (*E. primigenius*) and the other European species." "*Mastodon*, as having the widest distribution—Europe, Asia, and America—as well as from being the most generalised type of the family, may well be considered as the most ancient form of the group; its earliest occurrence in India is in the supra-nummulitic beds of Sind and Kach, and its latest existence was probably in the marshes of the Ohio, where it not unlikely lived down to the human period. It is the only American representative of the family, and its migration may well have taken place from India westward. Mastodon was the first of the elephants to die out in India, it being unknown after the Siwalik period." Part 3 includes a paper on "The Age of some Fossil Floras of India," by Ottocar Feistmantel; a note on "The Geological Age of Certain Groups Comprised in the Gondwána series of India, and on the evidence they afford of distinct Zoological and Botanical Terrestrial Regions in Ancient Epochs," by W. T. Blanford, F.R.S.; a memoir on "The Fossiliferous Strata at Matéri and Kotá," by T. W. H. Hughes, F.G.S.; and notes on the "Fossil Mammalian Faunæ of India and Burma," by R. Lydekker, B.A. Mr. Blanford concludes "that the faunas and floras of distant lands varied in palæozoic and mesozoic times, as they do at the present day far more than the fauna of the seas; in short, that there were distinct zoological and botanical provinces, and that evidence founded upon fossil plants of the age of rocks in distant regions must be received with great caution, and that such evidence is certainly in some cases opposed to that furnished by the marine fauna." Mr. Lydekker, in summing up the fossil mammalia of India, finds that all the species found in the Indian tertiaries below the Nerbudda beds are extinct. The numerical relations of the genera are as follows:—

Extinct	25
Peculiar to Indian tertiaries	14
Common to Indian and European tertiaries	26
Common to fossil and living Indian faunæ	17
Common to Indian tertiaries and modern Africa ..	12
Common to Indian tertiaries and modern Europe ..	8

"The greatest number of genera common to any two periods occur in the tertiaries of Europe and India; next to them the greatest common number is found in the living and fossil Indian faunæ; thirdly, a small number of genera is common to the extinct fauna of India and the living fauna of Africa; a few genera are common to the extinct Indian fauna and the modern European fauna; while a larger number of genera are common to the living faunæ of India and Africa." These results, the author considers, "appear clearly to point to some former connection by land between the continents of India, Africa, and Europe." A land connection between the two former regions stretching across the present Indian Ocean has been named Indo-oceania in a recent paper by Mr. H. F. Blanford ("Quarterly Journal of the Geological Society of London," November, 1875), and is known among other investigators as Lemuria. In those days it must be remembered that the peninsula of India was not connected with Central Asia, but was separated from it by a deep Eo-Miocene sea. The connection between India and Europe was of course by way of Africa.

Part 1 of vol. xii. of "Memoirs of the Geological Survey of India" contains an account of the geological features of the South Mahratta country and of the adjacent districts. The author, Mr. R. Bruce Foote, F.G.S., describes the various formations from the gneiss and the associated intrusive rocks to the subaërial formations and soils. He mentions a small natural lake near Sanageh, the only one in Southern India, but remarks—"There is one other reservoir of fresh water which I should unhesitatingly regard as a true lake if glacial phenomena were admissible in the peninsula of India." On this important point, the former glaciation of India, we do not find here any evidence. On the Shervaroy Hills peat forms largely at an elevation of above 4000 feet, and in the Wynád at much lower levels. This is an important fact, since the formation of peat within the tropics, except on very high mountains, has been declared impossible. In the lower iron-clay of Sadda caves occur, said to be very extensive: they have not, however, been explored. The economic geology of the district presents no striking features. They afford nothing of value except building-stone, some of very fine quality, and iron-ore, the demand for which is diminishing owing to the scarcity of fuel. There is an earthy form of peroxide of manganese found among the dolomite of Bhimgarh, and gold occurs in some of the streams flowing into the upper part of the Malprabha, but the yield is exceedingly small. The author failed in getting an appreciable quantity of gold in a number of carefully-selected samples of sand and gravel collected in promising places in the bed of the stream. The work is illustrated with sections and characteristic views of the scenery, many of which present very remarkable features.

Physics.—Continuing his researches on the "Molecular Pressure" theory of the repulsion resulting from radiation, Mr. Crookes, F.R.S., has constructed an instrument in which a movable fly is caused to rotate by the molecular pressure generated on fixed parts of the apparatus. This instrument, which is called the Otheoscope (ωθέω, I propel), he described to the Royal Society on April 26, 1877. While the glass bulb is an essential portion of the machinery of the radiometer, without which the fly would not move, in the otheoscope the glass vessel simply acts as a preserver of the requisite amount of rarefaction. Carry a radiometer to a point in space where the atmospheric pressure is equal to, say, one millimetre of mercury, and remove the glass bulb; the fly will not move, however strong the incident radiation. But place the otheoscope in the same conditions, and it will move as well without the case as with it. The following is a list of the otheoscopes Mr. Crookes has already made, together with some new experimental radiometers, which were exhibited at the Soirée of the Royal Society in May last:—

1. *Otheoscope.*—A four-armed fly carrying four vanes of thin clear mica is mounted like a radiometer in an exhausted glass bulb. At one side of the bulb a plate of mica blacked on one side is fastened in a vertical plane, in such a position that each clear vane in rotating shall pass the plate, leaving a space between of about a millimetre. If a candle is brought near, and by means of a shade the light is allowed to fall only on the clear vanes, no motion is produced; but if the light shines on the black plate, the fly instantly rotates as if a wind were issuing from this surface, and keeps on moving as long as the light is near.

2. *Otheoscope.*—A four-armed fly carries roasted mica vanes, and is mounted in an exhausted glass bulb like a radiometer. Fixed to the side of the bulb are three plates of clear mica, equidistant from each other in a vertical plane, but oblique to the axis. A candle brought near the fixed plates generates molecular pressure, which falling obliquely on the fly, causes it to rotate.

3. *Otheoscope.*—A large horizontal disk, revolving by the molecular disturbance on the surface of inclined metallic vanes, which are blacked on both sides in order to absorb the maximum amount of radiation.

4. *Otheoscope.*—Inclined aluminium vanes driven by the molecular disturbance from the fixed blacked mica disk below, blowing (so to speak) through them.

5. *Otheoscope.*—A large horizontal coloured disk, of roasted mica, driven by inclined aluminium vanes placed underneath it.

6. *Otheoscope.*—A bright aluminium disk cut in segments, and each segment turned at an angle, driven by a similar one below of lampblacked silver.

7. *Radiometer.*—A vertical radiometer, made with eight disks of mica blacked on one side, and the whole suspended on a horizontal axis which works in two glass cups. The motion of the radiometer is assisted on each side by driving vanes of aluminium blacked on one side.

8. *Radiometer.*—A vertical turbine radiometer, the oval vanes of roasted mica blacked on one side.

9. *Radiometer.*—A spiral radiometer of roasted mica blacked on the upper side.

10. *Radiometer* of large size, showing great sensitiveness.

11. *Radiometer.*—A two-disk radiometer, the fly carrying roasted mica disks blacked on one side; in front of each black surface is fixed a large disk of thin clear mica. The molecular disturbance set up on the black surface, and streaming from it, is reflected in the opposite direction by the clear plate of mica, causing the fly to move abnormally, *i.e.*, the black surface towards the light.

12. *Radiometer.*—A two-disk radiometer, the fly carrying roasted mica disks blacked on one side, similar to No. 11, but with a large clear disk on each side. The molecular disturbance, prevented from being reflected backwards by the second clear disk, is thus caused to expend itself in a vertical plane, the result being a total loss of sensitiveness.

13. *Radiometer.*—A two-disk, cup-shaped, aluminium radiometer, facing opposite ways; both sides bright. Exposed to a standard candle 3·5 inches off, the fly rotates continuously at the rate of one revolution in 3.37 seconds. A screen placed in front so as to let the light shine only on the convex surface, produces repulsion of the latter, causing continuous rotation at the rate of one revolution in 7·5 seconds. When the convex side is screened off, so as to let the light shine only on the concave, continuous rotation is produced at the rate of one revolution in 6·95 seconds, the concave side being apparently attracted. These experiments show that the repulsive action of radiation on the convex side is about equal to the attractive action of radiation on the concave side, and that the double speed with which the fly moves when no screen is interposed is the sum of the attractive and repulsive actions.

14. *Radiometer.*—A two-disk, cup-shaped, aluminium radiometer, lampblacked on the concave surfaces. In this instrument the usual action of light is reversed, rotation taking place, the bright convex side being repelled, and the black concave attracted. When the light shines only on the bright convex side, no movement is produced, but when it shines on the black concave side, this is attracted, producing rotation.

15. *Radiometer.*—A cup-shaped radiometer similar to the above, but having the convex surfaces black and the concave bright. Light shining on this instrument causes it to rotate rapidly, the convex black being repelled. No movement is produced on letting the light shine on the bright concave surface, but good rotation is produced when only the black convex surface is illuminated.

16. *Radiometer.*—A multiple-disk, cup-shaped, turbine radiometer, bright on both sides, working by the action of warm water below and the cooling effect of the air above.

17. *Radiometer.*—A four-armed metallic radiometer with deep cups, bright on both sides.

18. *Radiometer.*—A four-armed radiometer, the vanes consisting of mica cups, bright on both sides.

19. *Radiometer.*—A four-armed radiometer, having clear mica vanes. The direction of motion being determined by the angle formed by the mica vanes with the inner surface of the glass bulb.

An important improvement in the production of the electric light has

been made by M. Paul Jablochkoff, an officer in the Russian engineering service. M. Jablochkoff's invention is called the Electric Candle, and is said to be devoid of all those appliances and restrictions which have rendered the application of the ordinary electric light to general lighting purposes an impossibility. The electrodes employed are composed of two small slips of carbon placed side by side, insulated by a piece of *kaolin*. Kaolin, in its solid state, is an insulator offering high resistance to the electric current, but which, under the influence of a powerful electric current, becomes heated and liquefies, in which state it is no longer an insulator, but a conductor offering a slight resistance to the current, which, when passed through it in this condition, affords a light, soft, steady, and brilliantly white, although it may be coloured by mixing with the kaolin the colour required. No mechanism is required to regulate this light, which once set up continues to burn during the passage of the current until the carbons are consumed, when they are replaced by others. The electrical arrangement consists of an ordinary magneto machine sending positive and negative currents alternately. From this machine radiate wires by which the current evolved is conveyed to the buildings, or points, at which it is required for use. The illuminating arrangement is put in circuit with these wires, and on the current traversing the carbon electrodes, it fuses the kaolin and produces the light. Thus, given a means of producing the necessary electric current, any number of lights may be obtained from the same electro-motor; each dependent upon itself and all entirely independent of each other. Any one light may be brought into use at pleasure, and extinguished when required, by connecting, or disconnecting, the wires in connection with them; whilst a light consumed may be replaced by another with equal ease. A series of experiments illustrating the lighting capabilities of this invention took place on June 15, at the West India Docks. The electric power was produced by one of the Paris *Alliance* Company's magneto-electric machines, with thirty-two horse-shoe magnets of seven plates each, and worked by a small agricultural engine of about eight horse-power. A yard 150 feet by 70, and covered with an awning, was well lighted with four lamps, mounted on posts about 15 feet high, each of which were said to be equal to one hundred gas-lights, but the size of the gas-light was not specified. The lights were toned down by opal glass globes. Pearl type was legible at any part of the covered area. After burning for about twenty minutes the lights were extinguished, and four gas-lamps, with four powerful burners to each, were turned on, evidently with the intention of showing the difference between the orange colour of the gas-flame and the pure white of the electric lamp. The company then adjourned to a large warehouse at the top of an adjacent building, measuring about 50 yards long by 25 yards wide, which was lighted from the outside by three electric candles without any intervening globes. Two of these candles were placed at the side and one at the end, but being only breast high the shadows of persons passing in front of them greatly interfered with the experiments tried. One of the objects of this portion of the trial was to ascertain whether this light could be used for sampling various descriptions of produce and merchandise. Several experts were present, but owing to the position of the light being horizontal instead of vertical, there semed to be some doubt as to its value in the case of samples of coffee, grain, pepper, and similar commodities, inasmuch as the strong shadow cast horizontally by the individual grains of the produce under examination interfered materially with their colour—the particular tint of a coffee berry, for instance, being an important factor in the estimation of the value of a sample. It was far otherwise with a nnmber of samples of coloured alpaca goods. The most difficult colours to judge of by gas-light, or during foggy weather, are dark olive greens, puces, and blues. Next to these come the lightest shades of straw-yellow and cream-colour. In the first case the colours are not to be distinguished from black, and in the second from white; but under the electric light the darkest Navy blues and the lightest greys came out in their true tints, even to the eyes of the uninitiated. The company then adjourned to the quay below, alongside which a large barque had been moored. Here the practicability of lighting ships' decks and holds, and the adjoining wharfs, was

most satisfactorily demonstrated, as well as the portability of the light itself. The amount of heat given off by M. Jablochkoff's candle is comparatively small, the glass globe of one of the lanterns used for lighting up the ship having been found to be only just comfortably warm after having been lighted for twenty minutes. The last experiment tried was, scientifically speaking, the most interesting of the whole, demonstrating as it did that M. Jablochkoff has succeeded in entirely doing away with the necessity for using carbons for the electric light. His newest form of candle consists of a thin plate of his kaolin composition, about 1½ inches long by 1 inch broad, and about $\frac{1}{16}$th inch thick. The sides of the plate are inserted in grooves cut in the wires forming the electrodes of the battery, which project very slightly above the top of the plate. In order to light this new form of candle a bridge of ordinary graphite is carried along the top edge of the porcelain plate. The graphite become incandescent, causing the porcelain to melt, which then becomes a conductor. The graphite gradually disappears, and the melted portion of the porcelain becomes incandescent, gradually vapourising at the rate of a millimetre an hour. The light given out by the porcelain seems softer, mellower, and much more constant and steady, than that given off by the combination of carbon and porcelain: indeed, after five minutes' examination with black spectacles, we failed to discern anything more than a barely perceptible start at distant intervals.

A singular case of the production of heat has been communicated to the French Academy of Sciences by M. J. Olivier. A square rod of steel, 80 centimetres in length and 15 millimetres square, is grasped firmly by both the hands of the operator, one of the hands being placed in the middle of the rod, and the other at one end. The free extremity is strongly pressed against an emery wheel revolving very rapidly. After a few minutes the extremity thus rubbed becomes strongly heated: the hand placed in the middle of the bar does not experience any feeling of heat, but the one at the ether extremity is heated to such an extent that the operator is compelled to let go.

The official appreciation of scientific knowledge in our Colonies is admirably shown by the official reports recently issued by Colonel A. Brunel, the Commissioner of Inland Revenue for Canada. These are reports on weights and measures, and on the analysis of gas and food. A sure evidence of the continued material progress of a country is shown by its appreciation of scientific knowledge as applied to accurate measurement. For the maintenance in Canada of the British standards of length and weight, and for the issue to the local authorities, as well as to chemists and physicists there, of precise copies of these standards, there has been established at Ottawa a department under proper scientific direction, and provided with apparatus of the highest class. This department has also the appointment and control of the Inspectors whose duty it is to inspect the trade weights and measures. These numerous Inspectors are provided with weighing and measuring apparatus of the best design. From Colonel Brunel's report it appears that—"The analysis of gas and food is being carried out in the Dominion in accordance with the latest scientific experience. In Canada, as elsewhere, condiments, coffee, and milk appear to be largely and unwholesomely adulterated. Quinine wine, which is an article of great demand in Canada, is found as sold to be a highly alcoholised wine containing gentian and *nux vomica*, with 20 per cent of alcohol, and is therefore a powerful stimulant instead of being a simple tonic. The testing of gas supplied for lighting and heating purposes is also the duty of the Department at Ottawa, and for this purpose chemists have been appointed, and a large quantity of photometric apparatus obtained from Mr. Sugg, of Westminster. The law appears, however, scarcely to have come yet into active operation, and we regret to see that it is at present deficient in relation to the inspection of the illuminating power of gas. We are glad to find that Colonel Brunel has been so well supported in the difficult task he has initiated, and we trust that his Department may be imitated in other of our Colonies and Dependencies.

Microscopy.—The "Monthly Microscopical Journal" of May contains a valuable paper by Mr. Thomas Palmer, B.Sc., "On the Various Changes Caused in the Spectrum by Different Vegetable Colouring Matters." The paper is accompanied with several spectrum charts, and contains a detailed account of the processes used in preparing the various colouring matters for examination, and also accurate measurements, reduced to wave lengths, of the positions of the various absorption bands. Mr. Sorby, in his remarks on the paper, stated that vegetable colouring matters are much more complex than is generally supposed; most of them are undoubted mixtures of two or three kinds of matters, and even the chlorophyll, the green colouring matter alone, is composed generally of two green matters, which exist separately in certain plants. The line it would be most important to carry out would be, what were the chemical differences which gave rise to these changes in the spectra.

"Notes on Inclusions in Gems, &c.," by Isaac Lea, LL.D. This paper, published in the "Proceedings of the Academy of Natural Sciences, Philadelphia," and reprinted in the "Monthly Microscopical Journal," vol. xvii., p. 198, may be consulted with advantage by all persons interested in this subject. It contains the history of the cavities in crystals and their contents, from the paper by Sir Humphry Davy, published in the "Philosophical Transactions," 1822, to the present time; a full list of references is given, and it forms a valuable contribution to the bibliography of this interesting subject.

An ingenious modification of the achromatic condenser has recently been constructed by Messrs. Beck. It consists of an achromatic combination mounted in the usual fittings; over this slides a cylinder upon an eccentric rotating axis; the disk closing the upper end contains a series of holes, which can be brought accurately one by one over the lenses of the condenser by the rotation of the fitting; these apertures carry a series of lenses, by which a modification of the illuminating pencil is obtained. Lenses, achromatic or not, may be used to alter the aperture and focal length; spot lenses, central or marginal stops can all be employed with the greatest facility, rendering the variety of illumination attainable almost infinite.

A new and simple oblique illuminator has been contrived by the Rev. Lord S. G. Osborne, M.A., which he calls the "Exhibitor." The foundation consists of a "Darker" stage, as used for carrying selenite plates in polarising. Two counter sinkings are turned in the revolving ring, the top one rather larger in circumference and shallower than the lower one. Into the lower sinking is dropped a disk of blackened metal, with a small hemispherical lens mounted in the centre. Into the upper sinking are placed thin metal disks with certain apertures made in them; the front of these disks is just level with the face of the stage, the back close to the front of the disk holding the small lens. A fine screw is cut into the back part of the revolving ring, coming up just below the lower counter sinking; into this screws a brass ring carrying another hemispherical lens of the full size of the aperture. The screw movement has a milled edge, permitting the distance between the lenses to be regulated. The apertures may be made as found useful; one very successful consists of No. 1, a fine slit, in length about the aperture of the upper lens *in the centre;* No. 2, a similar aperture a little way, say not quite its own length from the centre; Nos. 3 and 4, a pin hole and a triangle, also a little out of the centre; these may all be cut in one disk. On the stage there are two steel springs for holding the sides, giving the means of shifting them in any direction. Abraham's achromatic prism is to be preferred to the mirror, and the flame of the lamp turned *edgeways*.

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I. OUR SIX-FOOTED RIVALS.

LET us suppose that, having no previous acquaintance with the subject, we were suddenly informed, on good authority, that there existed in some part of the globe a race of beings who lived in domed habitations, aggregated together so as to form vast and populous cities, —that they exercised jurisdiction over the adjoining territory, laid out regular roads, executed tunnels underneath the beds of rivers, stationed guards at the entrance of their towns, carefully removed any offensive matter, maintained a rural police, organised extensive hunting expeditions, at times even waged war upon neighbouring communities, took prisoners and reduced them to a state of slavery,—that they not merely stored up provisions with due care, to avoid their decomposition by damp and fermentation, but that they kept cattle, and in some cases even cultivated the soil and gathered in the harvest. We should unquestionably regard these creatures as human beings who had made no small progress in civilisation, and should ascribe their actions to reason. If we were then told that they were not men, and they were in some places formidable enemies to man, and had even by their continued molestations caused certain villages to be forsaken by all human occupants, our interest would perhaps be mixed with some little shade of anxiety lest we were here confronted by a race who, under certain eventualities, might contest our claim to the sovereignty of the globe. But when we learn that these wonderful creatures are insects some few lines in length our curiosity is cooled; we are apt, if duly guided by dominant prepossessions, to declare that the social organisation of these beings is not civilisation, but at most *quasi*-civilisation,—that their guiding principle is not reason, but "instinct,"

or *quasi*-intelligence, or some other of those unmeaning words which are so useful when we wish to shut our eyes to the truth. Yet that ants are really, for good or evil, a power in the earth, and that they seriously interfere with the cultivation and development of some of the most productive regions known, is an established fact. A creature that can lay waste the crops of a province or sack the warehouses of a town has claims upon the notice of the merchant, the political economist, and the statesman, as well as of the naturalist.

Many observers have been struck with the curious mixture of analogies and contrasts presented by the Annulosa and the Vertebrata. These two classes form, beyond any doubt, the two leading subdivisions of the animal kingdom. To them nineteen-twentieths of the population of the dry land, both as regards individuals and species, will be found to belong, and even in the world of waters they are largely represented. At the head of the Vertebrata stands the order of the Primates, culminating in man. At the head of the Annulosa the corresponding place is taken by the Hymenopterous insects. It is very remarkable—as first pointed out, we believe, by Mr. Darwin—that these two groups of animals made their appearance on the earth simultaneously. But along with this analogy we find a contrast. Man stands alone among the Primates as a socially organised being, possessing a civilisation. Among the Hymenoptera the lead is undoubtedly taken by the ants, which, like man, have a brain much more highly developed than that of the neighbouring inferior groups. But there is no one species of ant which enjoys a pre-eminence over its congeners anything at all approaching in its nature and extent to man's superiority over the gorilla or the mias. What may be the cause of this contrast we know not. Perhaps it is merely due to the tendency of the Annulosa to branch out into a scarcely numerable host of forms, whilst the vertebrate structure, less plastic, lends itself more sparingly to variation. Perhaps, on the other hand, lower human or higher ape-forms than any now existing have been extirpated, as the traditions of many ancient nations would seem to admit.

At any rate, whilst the superiority of the ants as a group to the remaining Hymenoptera, to all other insects, and to the rest of the annulose "sub-kingdom" is undisputed, we are unable to decide which species of ant is elevated above the rest of the Formicide family. Possibly more extended and more systematic observations may settle this interesting question. According to our present knowledge the claims

of the agricultural ant, of Western Texas (*Myrmica barbata*), seem perhaps the strongest. This species, which has been carefully studied by Dr. Lyncecum, for the space of twelve years, is, save man, the only creature which does not depend for its sustenance on the products of the chase or the spontaneous fruits of the earth. As soon as a colony of these ants has become sufficiently numerous they clear a tract of ground, some 4 or 5 feet in width, around their city. In this plot all existing plants are eradicated, all stones and rubbish removed, and a peculiar species of grass is sown, the seeds of which resemble very minute grains of rice. The field—for so we must call it—is carefully tended by the ants, kept free from weeds, and guarded against marauding insects. When mature, the crop is reaped and the seeds are carried into the nest. If they are found to be too damp they are carefully carried out, laid in the sunshine till sufficiently dry, and then housed again. This formation of a plot of cleared land—or, as Dr. Lyncecum not very happily terms it, a pavement, is a critical point in the career of a young community. Any older and larger city which may lie within some fifty or sixty paces looks upon the step as a *casus belli*, and at once marches its armies to the attack. After a combat, which may be prolonged for days, Providence declares in favour of the largest battalions, and the less numerous community is exterminated, fighting literally to the last ant. Where a colony is unmolested it increases rapidly in population, and undertakes to lay out roads: one of these, from 2 to 3 inches in width, has been traced to a distance of 100 yards from the city. These ants are not very carnivorous, nor do they damage the crops of neighbouring farmers. Persons who intrude upon the "pavement" are bitten with great zeal, but otherwise the species may be regarded as harmless. One creature alone they seem to tolerate on their "pavement,"—the so-called small black "erratic" ant,—which, as Dr. Lyncecum conjectures, may be of some use to them, and which is therefore allowed to build its small cities in their immediate neighbourhood. If it becomes too numerous, however, it is got rid of, not by open war, but by a course of systematic and yet apparently unintentional annoyance. The agricultural ants suddenly find that it is necessary to raise their pavement and enlarge the base of their city. In carrying out these alterations they literally bury the nests of their neighbours under heaps of the small pellets of soil thrown up by the prairie earthworms, and continue this process till the erratic ants in sheer despair remove to a quieter spot.

Concerning the government either of the agricultural ants or of other species our knowledge is of a very negative character. The queens, or rather mothers, of the city are indeed treated with great attention, but their number is quite indefinite, and, unlike female hive-bees, no jealousy exists between them. How their migrations, their wars, their slave-hunts are decided on, or even how the guards on duty are appointed, and the visiting parties selected who go round to inspect the works, and who sometimes insist on the destruction and rebuilding of any badly-executed portion, we are utterly ignorant. The outer manifestations of ant-life we have to some extent traced, but its inner springs, its directing and controlling powers have eluded our observation.

It has been remarked in the "Quarterly Journal of Science" that ants, unlike man, have solved the problem of the practical organisation of communism: this is literally true. In a formicary we can detect no trace of private property; the territory, the buildings, the stores, the booty, exist equally for the benefit of all. Every ant has its wants supplied, and each in return is prepared to work or to fight for the community as zealously as if the benefit of such toil and peril were to accrue to itself alone. If the principle —so common among men—that there is no harm in robbing or defrauding a municipal body, or the nation at large, crops up in an ant-hill at all, it must evidently be stamped out with an old-fashioned promptitude. But to understand why the ant has succeeded where man has failed, we must turn to certain fundamental distinctions between human and ant society, or perhaps, speaking more general, between the associations of vertebrate and those of annulose animals. A human tribe or nation—and in like manner, *e.g.*, a community of beavers or of rooks—is formed by the aggregation not of single individuals, but of groups, each consisting of a male, a female, and their offspring. The social unit among vertebrates, therefore, is the family, whether permanent or temporary, and whether monogamous or polygamous. In numberless cases the family exists without combining with other families to form a nation, but we greatly doubt if there exists a single case of a vertebrate nation not formed of and resolvable into families.

Among the Annulosa this is reversed. The family among them scarcely exists at all. Rarely is the union of the male and the female extended beyond the actual intercourse, all provision for the future young devolving upon the latter alone. Among the rare exceptions to this rule we may

mention the burying-beetle and some of the dung-beetles, both sexes of whom labour conjointly to find and inter the food in which the eggs are to be deposited. Generally speaking, moreover, the young insect never knows—never even sees—its parents, who in most cases have died before it has emerged from the egg. Among non-social insects the earwig and a few other Orthoptera form the chief exceptions. Where a regularly organised society, a nation, or tribe exists among annulose animals, it is not formed by the coalescence of families to a higher unity. The family, if it can be said to exist at all, is conterminous and identical with the nation. This absence of a something whose claims are felt by all ordinary men to be stronger than those of the State has rendered the successful organisation of the "Commune" feasible among ants, and among other social Hymenoptera, such as bees, wasps, &c. With them the State has no rival, and absorbs all the energies which in human society the individual devotes to the interests of his family. We thus see that theorists on social reform have been, from their own point of view, logically consistent in attacking the institution of marriage and the whole system of domestic life: they have sought to abolish the great impediment to the Commune, and to approximate man to the condition of our six-footed rivals, and to constitute society not as heretofore of molecules, but of atoms.

But it is not enough to show that the failure of Communism among mankind and its success among certain Hymenopterous insects are due to the existence and the power of the family in the former case, and to its absence in the latter. We have yet to enquire into the wherefore of so important a distinction. Vertebrate society, where it exists at all, is founded on family life, because every vertebrate animal is sexual, and as such is attracted to some individual of the opposite sex by the strongest instinct of its nature, that of self-preservation alone excepted. Invertebrate society, where it exists in perfection, as among the Hymenoptera, is not formed by a union of families, because the great majority of Hymenopterous individuals (in the social species) are non-sexual, neuter, incapable of any private or domestic attachments, and devoted to the community alone. To attempt, without the existence of such an order, to introduce the social arrangements of the ant —*i.e.*, Communism—among mankind is as futile and as irrational as the endeavour to fly without wings: the very primary conditions for success are wanting.

It may not be amiss to examine a little further in the

same direction. Among men there is a great diversity both in intellect and in energy. The more highly-endowed individual, if he does not leave his children in a better position, materially speaking, is likely to transmit to them his own personal superiority. In this manner the theoretical equality assumed as one of the bases of Communism is in practice annihilated. Among ants nothing of this kind can prevail. The workers and the fighters are sexless. If any individual is superior to its fellows in strength or in intelligence—and we have every reason to believe that such must be the case—it has no posterity to whom its acquisitions could be bequeathed or its personal superiority handed down. Hence the formation of an aristocracy is impossible, and whatever benefit may result from the labours of such an exceptional individual flows to the entire community. In the converse manner the formation of a pariah-, a criminal-, or a pauper-class is frustrated, and the public is not burdened with useless or dangerous existences.

It is indisputable that this arrangement, joined to the brief term of insect-life, must greatly retard the progress of the ant in civilisation. It has been remarked that were human life longer our development in knowledge and in the arts would be much more rapid. Take our present condition: by the time a man has completed his education, general and special,—has fully developed his own mental faculties and mastered the position of the subject he has selected,—he will be rarely less than five and twenty years of age. By the time he is fifty, as a rule, his power of origination begins to decline, and the remainder of his life is spent more in completing and rounding off the work of his younger days than in making fresh inroads into the unknown. Did our full vigour of intellect extend over a century, instead of over a fourth of that duration, we should undoubtedly effect much more. On the other hand, a shortening of our time of activity would have a powerfully retarding effect on the career of discovery and invention. Can we then wonder if the short-lived ant and bee sometimes appear to us stationary in their civilisation? But this very brevity of the career of each individual acts decidedly in favour of the preservation of social equality. If either ant or man is disposed to rise or to fall, then the shorter the time during which such rise or fall is possible the better will the uniform level of society be preserved. To prevent misunderstanding we must remark that castes with a corresponding difference of duties, and, according to some authorities, with a diversity of honour also, do occur in the

ant-hill; but within each caste all are on an exactly equal footing.

If we compare the zoological rank of our "six-footed rivals" with our own, we must, from one point of view, concede them a higher position. The more perfectly developed is any animal the more do we find it possessed of an especial organ for the discharge of every function. In like manner it may be contended that, as a species rises in the scale of being, duties once indiscriminately performed by all the species are assigned to distinct individuals. Among the humbler groups of the animal kingdom the whole reproductive task is performed by all members of the species. In other words, hermaphroditism prevails. As we ascend to higher groups the sexes are separated, and the species becomes dimorphous. This arrangement prevails among all vertebrate animals, and among a large majority of annulose species. We find here already, however, one of those contrasts which so often prevail between these two great series of beings. Among vertebrates, and especially in mankind, the function of the female sex seems limited to the nurture —intra- and extra-uterine—of the young. Were man immortal and non-reproductive, woman's *raison d'etre* would disappear. Among Annulosa the very reverse holds good; the females are as a rule larger, stronger, and more long-lived, whilst the task of the male seems limited to the fecundation of the ova. This being once performed, his part is played. Among butterflies, moths, and ants his death speedily follows, whilst among spiders he is generally killed and devoured by his better-half. This predominance of the female sex seems to prepare the way for the phenomenon which we recognise among the social Hymenoptera. Here the species become no longer dimorphous, but polymorphous. In other words, in addition to the males and females, whose task is now exclusively confined to the mere function of reproduction, there are, as we have seen, one or more forms of females, sexually abortive, but so developed in other respects as to form the castes of workers and fighters, upon whom the real government of the ant-hill belongs, who provide for its enlargement, well-being, and defence.

It may, we think, be legitimately contended that the development of a distinct working order is a step in advance similar to that taken by the distribution of the sexual functions among two different individuals—that the polymorphic species is higher than the dimorphic, just as the dimorphic is higher than the monomorphic.

Of the development of a neuter order among vertebrate animals, and especially among mankind, we know nothing which can be fairly called a trace. But in comparing the two civilisations, that of man and that of the ant, we must be struck with the fact that the former has from time to time imitated this peculiar feature. The attempts, however, whether made by the devotion of certain classes to celibacy or by actual emasculation, have been as unsuccessful as the sham elephants of Semiramis. Celibates retaining the sexual appetite, but deprived of its legitimate exercise, have always been a disturbing force in society. On the other hand, emasculation, instead of—as might have been perhaps, *a priori*, anticipated—increasing the powers of body and mind, enfeebles both. What would be the moral and social effects of the appearance of a neutral form of the human species analogous to the working-bee or ant it is impossible to foresee; but we may venture to surmise that they would not be entirely desirable.*

It may be suggested that the institution of caste among so many human races is an adumbration of the natural castes existing among social insects, each devoted to some especial function.

The remarkable intelligence of ants has from very early ages made a profound impression on man. Cicero considered them possessed of "mind, reason, and memory."† To the present day those who watch the formicary, not in order to defend prepossessions, but to arrive at truth, come to the same conclusion, unpopular though it may be. We sometimes wonder whether ants, like men, consider themselves the sole reasonable beings on the globe, prove their position by sound *a priori* arguments, and accuse those who take a different view of "scepticism" or "agnosticism."

When it is no longer possible to meet with a flat denial all instances of correct inferences drawn and of happy contrivances adopted by brutes in general and by ants in particular, the writers who still claim reason as the exclusive prerogative of man bring forward a curious objection: they urge that we should likewise collect proofs of animal folly and stupidity, and seem to think that these latter instances would nullify any conclusion that might be drawn from the former. That instances are numerous where some animal

* It is very remarkable that among the Termites, which though improperly called "white ants" belong to a different order of insects, neuters exist. These, however, do not appear to be imperfectly developed females. It would thus seem that among insects social organisation necessitates a class of sexless individuals.

† "Mens, ratio, et memoria."

fails to draw an inference—very obvious, in our view—or to adopt some very simple expedient we do not deny, and that their conduct hence seems strangely chequered we admit. What, *e.g.*, can seem more inconsistent than the following cases? Sir John Lubbock, to test the intelligence of ants, placed a strip of paper so as to serve as a bridge or ladder for some ants which were carrying their pupæ by a very roundabout away. The slip was, however, purposely left short of its destination by some small fraction of an inch. It would have been very easy for the ants either to have dropped themselves and their burden down this short distance, or to have handed the pupæ to the other ants below, or to have piled up a small amount of earth from below so as to meet the slip of paper, and thus make the descending road continuous. They adopted, however, none of these expedients, but continued to travel the roundabout way.

On the other hand, Mr. Tennant tells us that *Formica smaragdina*, in forming its dwellings by cementing together the leaves of growing trees, adopts the following method:—A line of ants, standing along the edge of one leaf seize hold of another, and bring its margin in contact with the one on which they are posted. They then hold both together with their mandibles, whilst their companions glue them fast with a kind of adhesive paper which they prepare. If the two leaves are so far apart that a single ant cannot reach from one to another, they form chains with their bodies to span over the gap. The same author also informs us that certain Ceylonese ants, when carrying sand or dry earth for the construction of their nests, glue several grains together so as to form a lump as large as they can carry, and thus economise time and labour.

Mr. Belt, in his "Naturalist in Nicaragua" (p. 27), gives the following account of the manner in which the *Ecitons*, or foraging ants of Central and South America, deal with what may be called engineering difficulties:—"I once saw a wide column trying to pass along a crumbling, nearly perpendicular slope. They would have got very slowly over it, and many of them would have fallen, but a number having secured their hold and reaching to each other remained stationary, and over them the main column passed. Another time they were crossing a water-course along a small branch, not thicker than a goose-quill. They widened this natural bridge to three times its width, by a number of ants clinging to it and to each other on each side, over which the column passed three or four deep; whereas, except for this expedient, they would have had to pass

over in single file, and treble the time would have been consumed."

Again, *Eciton legionis*, according to Mr. Bates, when digging mines to get at another species of ant whose nests they were attacking, the workers are divided into parties, "one set excavating and another set carrying away the grains of earth. When the shafts became rather deep the mining parties had to climb up the sides each time they wished to cast out a pellet of earth, but their work was lightened for them by comrades who stationed themselves at the mouth of the shaft and relieved them of their burdens, carrying the particles with an appearance of foresight which quite staggered me, a sufficient distance from the edge of the hole to prevent it from rolling in again."

What, then, are we to learn from these somewhat inconsistent cases? Are we to conclude that Sir John Lubbock, Mr. Belt, Mr. Bates, and Mr. Tennant must be careless and incompetent observers? Assuredly not. Are we to believe that ants are stupid, irrational creatures, and that when they do anything right it must be regarded as an accident or ascribed to that convenient phantom, instinct? Still less: the well-established cases which are on record agree badly with either of these suppositions. The true explanation of the difficulty is that, like all finite intelligences, ants are not equally wise on all occasions. Sometimes they hit upon the best expedient for evading or overcoming an obstacle, but sometimes, under circumstances not more complicated, they fail. This is doubtless the case with man himself. If contemplated by some being endowed with higher reasoning powers, would he not be pronounced a most curiously inconsistent mixture of sagacity and stupidity, now solving problems of no small difficulty, and now standing helpless in presence of others even more simple? That such is in reality the case with man is proved by the history of discoveries, and of their reception. Do we not always say when we hear of any great step, whether in scientific theory or in the practical arts, "How simple, how natural!" Yet, simple and natural as it is, all sorts and conditions of men lived for centuries without opening their eyes to it. To those who, on the score of incidental blunders and stupidities, deny the rationality of animals, we would hold up the ever-memorable "egg" of Columbus, and exclaim "What, gentlemen, do you expect the ant to be more uniformly and consistently intelligent than your erudite selves?"

Concerning the language of ants no small diversity of opinion has prevailed; but among actual observers the

general conclusion is that these tiny creatures can impart to each other information of a very definite character, and not merely general signals, such as those of alarm. It has been found that ants fetched by a messenger for some especial purpose seem, when they arrive at the spot, to have some knowledge of the task which is awaiting them, and set about it at once without any preliminary investigation. The cases which we quote elsewhere from Mr. Belt are very conclusive on this point. In order to decide whether ants are really fetched to assist in tasks beyond the strength of any one of their number, Sir John Lubbock instituted a very interesting and decisive experiment. It is well known that if the larvæ of ants are taken out of the nest, the workers never rest till they have fetched them back. Sir John Lubbock took a number of larvæ out of his experimental formicary, and placed them aside in two parcels very unequal in number. Each of these lots was soon discovered by an ant, who at once fell to work to carry the larvæ back to the nest, and was soon joined by others, eager to assist. The observer reasoned thus:—If these ants have come to the spot by accident, it is probable that the number who arrive at each lot will be approximately equal. On the other hand, if they are intentionally fetched to assist in removing the larvæ, the number in each case will most likely bear some proportion to the amount of work to be done. The result was that the large heap of larvæ was visited by about three times as many ants as the small one. Hence the inference is plain that ants can call assistance to any task in which they are engaged, that they can form some estimate of the amount of labour that will be required, and can make their views in some manner known to their companions. The manner in which, when on the march, they are directed by their officers, and the promptitude and precision with which a column is sent out to seize any booty indicated by scouting parties, show likewise a completeness and precision of language very different from anything we observe in quadrupeds and birds.

But as to the nature of this language, which Mr. Belt rightly calls "wonderful," we are as yet very much in the dark. Sounds audible to our ears they scarcely can be said to emit. Their principal organs of speech are doubtless the antennæ: with these, when seeking to communicate intelligence they touch each other in a variety of ways. There can be no doubt that, with organs so flexible and so sensitive, an interchange not merely of emotions but of ideas must be easy.

But there is another channel of communication which deserves to be carefully investigated. We know that the language of vertebrates, or at least of their higher sections, turns on the production or recognition of sounds. What if the language of social insects should be found to depend, in part at least, on the production and recognition of odours? We have already full proof that their sense of smell is developed to a degree of acuteness and delicacy which utterly passes our conceptions of possibility, and to which the scent of the keenest hound presents but a very faint approximation. Collectors of Lepidoptera are well aware that if a virgin female moth of certain species is enclosed in a box, males of the same species will make their appearance from distances which may be relatively pronounced prodigious. As soon, however, as the decoy has been fecundated this attraction ceases. This is only one among the many phenomena which testify to the wonderful olfactory powers of insects. So much, then, for the recognition of odours. Nor is their production among insects a matter open to doubt. Scents, distinctly perceptible even to our duller organs, are given off by many. The pleasant odour of the musk-beetle, and the offensive smells of the ladybirds, the common ground-beetles, the oil-beetles, the Spanish fly, and the "devil's coach-horse"—hence technically named *Gœrius olens*—are known to every tyro in entomology. The next question is, Are these odours at all under the control of the insect, and capable of being produced, suppressed, or modified at will? We have noticed many instances where the odours of insects became more intense under the influence of anger or alarm. A peculiarly pungent odour is said to issue from a bee-hive if the inmates are becoming excited.

The possibility of a scent-language among insects must therefore be conceded. Mr. Belt thinks that the *Ecitons* mark out a track which is to be followed by their comrades by imparting to it some peculiar odour. He says:—"At one point I noticed a sort of assembly of about a dozen individuals that appeared in consultation. Suddenly one ant left the conclave, and ran with great speed up the perpendicular face of the cutting without stopping. It was followed by others, which, however, did not keep straight on like the first, but ran a short way, then returned, and then again followed a little farther than the first time. They were evidently scenting the trail of the pioneer, and making it permanently recognisable. These ants followed the exact line taken by the first one, though it was far out of sight. Wherever it had made a slight *détour*, they did

so likewise. I scraped with my knife a small portion of the clay on the trail, and the ants were completely at fault for a time which way to go. Those ascending and those descending stopped at the scraped portion, and made short circuits until they hit the scented trail again, when all their hesitation vanished, and they ran up and down it with the greatest confidence."

That among groups like the *Ecitons*, in which the sense of sight is imperfect or even totally wanting, enhanced delicacy of scent and touch must be required in compensation may be taken as self-evident. With the language of ants, and especially with a possible scent-language, is connected the faculty by means of which denizens of the same city recognise each other under circumstances of great difficulty. In the battles which take place between two nations of the same species, how, save by scent, do the tiny warriors distinguish friend from foe? We are told by some older observers that if an ant is taken from the nest, and restored after the lapse of several months, it is at once received by its companions and caressed, whilst a stranger ant introduced at the same time is rejected, and generally killed. To a great extent this has been confirmed by recent investigators. The returned exile was not indeed caressed, but was quietly allowed to enter the nest, whilst a stranger was at once greeted with hostile demonstrations. It has been maintained that this power of recognition is destroyed by water, and that ants will treat a comrade as an enemy if he has received a drenching. This, however, is evidently a mistake. To prevent rain from penetrating into the nests of the agricultural ant the guards block up the doorways with their bodies, and are often drowned at their posts. But their companions are not thereby prevented from recognising them, as they try to bring the dead bodies to life.

Even more wonderful than the mere intelligence of the ant is its power of organisation—the point, probably, in which it approaches most closely to man. Suppose that ants, instead of forming nations, lived like most creatures, merely in pairs, each endeavouring to rear a young brood, who when mature would enter upon a similarly isolated career. Let them be as brave, as intelligent, and as strong as they now are, still how humble and insecure would be their position! Against the attacks of the giant spiders, centipedes, hornets, and wasps of warm climates they could make no effectual resistance. Prey which in their present condition they easily secure would escape them, or would

scarcely even notice their puny efforts. In short, there is every reason to believe that many of their species would become extinct, and that the remainder would live, so to speak, mainly on sufferance, playing no appreciable part in the economy of the globe. Turning from this hypothetical survey of the ant as an individual, unorganised being, to its actual condition, we see the most striking contrast. Mr. Belt gives the following graphic account of the excitement caused by a marching column of *Ecitons* in the primeval forests of Nicaragua:—"My attention was generally first called to them by the twittering of some small birds belonging to different species. On approaching, a dense body of the ants, 3 or 4 yards wide and so numerous as to blacken the ground, would be seen moving rapidly in one direction, examining every cranny and underneath every fallen leaf. On the flanks and in advance of the main body smaller columns would be pushed out. These smaller columns would generally first flush the cockroaches, grasshoppers, and spiders. The pursued insects would rapidly make off, but many, in their confusion and terror, would bound right into the midst of the main body of ants. At first the grasshopper, when it found itself in the midst of its enemies, would give vigorous leaps, with perhaps two or three of the ants clinging to its legs. Then it would stop a moment to rest, and that moment would be fatal, for the tiny foes would swarm over the prey, and after a few more ineffectual struggles it would succumb to its fate and soon be bitten to pieces and carried off to the rear. The greatest catch of the ants was, however, when they got amongst some fallen brushwood. The cockroaches, spiders, and other insects, instead of running right away, would ascend the fallen branches and remain there whilst the host of ants were occupying all the ground beneath. By-and-bye up would come some of the ants, following every branch and driving before them their prey to the ends of the small twigs, where nothing remained for them but to leap, and they would alight in the very throng of their foes with the result of being certainly caught and pulled to pieces.

"The moving columns of *Ecitons* are composed almost entirely of workers of different sizes, but at intervals of 2 or 3 yards there are larger and lighter-coloured individuals that often stop and sometimes run a little backward, stopping and touching some of the ants with their antennæ. They look like officers giving orders and directing the march of the column.

"The ants send off exploring parties up the trees, which

hunt for nests of wasps, bees, and probably birds. If they find any they soon communicate the intelligence to the army below, and a column is sent up immediately to take possession of the prize. I have seen them pulling out the larvæ and pupæ from the cells of a large wasps' nest, whilst the wasps hovered about, powerless, before the multitude of the invaders, to render any protection to their young."

Still more formidable are the "driver ants" of Tropical Africa, so called because on their approach even the lion, the elephant, and the huge python at once betake themselves to flight.

Nor are the purely vegetarian ants of less importance in the economy of the countries they inhabit. They decide, in a manner, what trees shall grow and what shall be exterminated, and it is only such as are comparatively distasteful to them that escape. In Nicaragua they render the acclimatisation of any foreign tree or vegetable a task of great difficulty. Mr. Belt was often told, on asking the reason why no fruit-trees were grown at certain places, "It is of no use planting them; the ants eat them up." These ants climb up the trees, when "each one, stationing itself on the edge of a leaf, commences to make a circular cut from the edge with its scissor-like jaws, its hinder feet being the centre on which it turns. When the piece is nearly cut off it is still stationed upon it, and it looks as though it would fall to the ground with it; but on being finally detached the ant is generally found to have hold of the leaf with one foot, and soon righting itself, and arranging its burden to its satisfaction, it sets off at once on its return."

An observer standing near the ant-hills "sees from every point of the compass ant-paths leading to them, all thronged with the busy workers carrying their leafy burdens. As far as the eye can distinguish their tiny forms, troops upon troops of leaves are moving up towards the central point and disappearing down the numerous tunnelled passages. The ceaseless toiling hosts impress one with their power, and one asks—What forests can stand before such invaders?" Concerning the use to which the ant-leaves are put some difference of opinion prevails; that they do not directly serve as food is admitted. Mr. Bates, from observations made in Brazil, concludes that "the leaves are used to thatch the domes which cover the entrances to their subterranean dwellings, thereby protecting from the deluging rains the young brood in the nests beneath." Mr. Belt, who has carefully examined the habits of an allied species in Nicaragua, believes that the real use they make of them

is as a manure, on which grows a minute species of fungus on which they feed,—that they are, in reality, mushroom growers and eaters. The reasons for this view are given in detail in Mr. Belt's work, and appear very satisfactory. But Mr. Bates's view may be correct also. In short, save man alone, there is no creature which can effect such widespread and profound alterations in the condition of a country as the tiny ant. It has been indeed mentioned in the "Quarterly Journal of Science" that the pig, the goat, and the rabbit have succeeded in extirpating the natural flora, and consequently to a great extent the fauna, of certain islands, such as St. Helena. Yet this takes place only in countries where there are no carnivorous beasts, birds, and reptiles to keep them in check. But in every warm and fruitful climate the ant is king. This power we perceive is not due to mere numbers; it is in great part the result of organisation. Other species of insects are perhaps even more numerous, and, individually considered, as capable of destructive action; but locusts, potato-beetles, mosquitoes, noisome as they may be considered, are, in comparison with ants, what a promiscuous mob is in comparison with a well-trained and organised army. Each ant, like an experienced soldier, knows—whether rationally or instinctively it matters not—that it will be systematically supported by its comrades. What would be the prospects of agriculture in Western Asia, in Northern Africa, or in the Western States of the American Union, if the locusts when engaged in desolating a field were to attack, *en masse*, any man or bird who should interfere with them? But, on the contrary, they allow themselves to be slaughtered in detail, each indifferent to the fate of his neighbour.

Ants evince that close mutual sympathy which to an equal extent can be traced probably in man alone, and which has in both these cases proved one of the primary factors in the development of civilisation. Had man been devoid of this impulse he would have remained a mere wandering savage—perhaps a mere anthropoid, occurring as a rare species in equatorial districts. Without a similar impulse the *Ecitons* would have ranked among the many solitary species of Hymenoptera. Of the mutual helpfulness of these same *Ecitons* Mr. Belt gives us some most interesting cases which came under his own observation:—"One day when watching a small column of these ants (*Eciton hamata*) I placed a little stone on one of them to secure it. The next that approached, as soon as it discovered its situation, ran backwards in an agitated manner, and soon

communicated the intelligence to the others. They rushed to the rescue : some bit at the stone, and tried to move it ; others seized the prisoner by the legs, and tugged with such force that I thought the legs would be pulled off—but they persevered until they got the captive free. I next covered one up with a piece of clay, leaving only the ends of the antennæ projecting. It was soon discovered by its fellows, which set to work immediately, and by biting off pieces of the clay soon liberated it. Another time I found a very few of them passing along at intervals : I confined one of these under a little piece of clay, with his head projecting. Several ants passed it, but at last one discovered it and tried to pull it up, but could not. It immediately set off at a great rate, and I thought it had deserted its comrade, but it had only gone for assistance, for in a short time about a dozen ants came hurrying up, evidently fully informed of the circumstances of the case, for they made directly for their imprisoned comrade, and soon set him free. The excitement and ardour with which they carried on their exertions for the rescue could not have been greater if they had been human beings."

Such cases as these are of the greater moment because many other social and semi-social animals treat an unfortunate companion in a very different manner. It is on record that a rook, which had got entangled among the twigs of a tree, was pecked and buffeted to death by its neighbours, despite the efforts of its mate for its protection.

Facts are not wanting which show that the social organisation of ants takes cognizance of sanitary matters. In Australia they have been known to bury their dead, not without some degree of formality* according to their caste. In experimental formicaries in this country ants have been observed to throw the bodies of their dead companions into the water surrounding their dwellings. In the nests of almost all species great care is taken to preserve cleanliness. The agricultural ant of Texas removes any offensive matter placed near its city, and will even take the trouble to carry away the droppings of cattle that have fallen on its cleared ground. Any dung-rolling beetle which brings its ball of ordure within these sacred precincts is at once attacked and put to death, and the nuisance is quickly cut to pieces and carried to a distance.

Nor are laws on other matters wanting. Ants who have, from some unknown cause, refused to work have been

* Journal of Linnean Society, vol. v., p. 217.

observed to be put to death. Among the agricultural ants prisoners have been known to be brought in by a fellow-citizen and handed over in a very rough manner to the guards who are always on duty on the level ground before the city, and who carry off the offender into the underground passages. What is his after fate is not known. It is almost needless to point out that even the faintest rudiment of law proves the existence of some notions of right and wrong, as well as of a power of communication which must go into minute details.

We have now to deal with the great question whether the civilisation of ants, like that of man, has been gradually and slowly developed by the accumulation of experience, or whether—as the believers in the fixity of habits and instincts still contend—it is primordial, coexistent with the species in all the details which we now observe. Direct historical evidence is here yet more difficult to obtain than as concerns animal structure. We smile, with just reason, at the French *savants* of the Egyptian expedition who imagined that by the study of the animal-mummies there preserved they might gain some light on—or rather find some argument *against*—the mutation of species. At the same time we readily admit that could we find a complete series of skeletons, anatomical preparations, or even photographs of the best-known animals made at intervals of a century and extending backwards for say a hundred thousand years, the doctrine of evolution would be brought to a crucial test. But concerning the former habits and instincts of animals correct information is far more difficult to obtain. The "stone book" is silent or oracularly vague. Even if we had written documents left us by some naturalist of the Miocene ages—if we can suppose such a being to have existed—what security should we have for the accuracy and the completeness of his researches?

To meet this difficulty an attempt remarkable for its subtle ingenuity has been made by Prof. Heer. He points out that, according to the reckoning of the most discreet geologists, at least a thousand centuries must have elapsed since Britain was severed from the continent of Europe. For this long stretch of time, therefore, British animals must have been cut off from their representatives in France, Belgium, and Switzerland. If, then, the habits of a certain slave-holding ant (*Formica sanguinea*) in England are found identical, as he maintains, with the habits of the same species in Switzerland, there is a strong presumption that its economy has undergone no change for the last hundred

thousand years. To this argument we must reply that the isolation between British and Continental species of insects is by no means so complete as is here assumed. Winged ants travel very considerable distances, and, if our memory does not deceive us, have been met with out at sea. That a part of a swarm should be blown over from the French coast to England, or *vice versâ*, is by no means improbable. And it is well known that if a party of working ants fall in with an impregnated female of their own species, they immediately lead her to their nest and install her in a royal apartment. That there may have been within the last ten thousand—or even one thousand—years direct intercommunication of this kind between the slave-making ants of England and those of Switzerland seems to us fully more probable than the contrary supposition.

Again, we may ask whether the conditions under which ants would be respectively placed in Switzerland and in England are not so closely analogous that their social development must proceed on parallel lines? In both they would encounter nearly the same climate, the same food, and the same enemies. Surely, therefore, a close correspondence in habits is no decisive proof of their immobility. But, after all, is there such an absolute accord between the habits of the Swiss and of the British ants as the validity of Professor Heer's argument would require? Mr. Darwin thinks that in the nests of the British *Formica sanguinea* there is a relatively smaller proportion of slaves, which therefore play a less important part in the economy of the ant-hill. Messrs. Kirby and Spence record a fact which, isolated as it is, seems to us to overthrow altogether the hypothesis of absolute stationariness. Ants have been found, namely, to establish their nest in the interval between the double casing of a glass bee-hive. Now, as such bee-hives are artificial objects and of very recent origin, they cannot have come in the way of the ants for any great length of time. They offered, however, a certain advantage in the uniform temperature and the shelter which they supplied. This fact must have been recognised by some prying ant, and the discovery being communicated to its comrades was turned to practical account. Is not this case the exact parallel of a step in the development of human civilisation? And if, as we see, ants can in one case observe a phenomenon, reason on such observation, and work out their conclusions in their daily life, we can certainly see no grounds for supposing that such processes may not have occurred often. In the case of larger animals, where observation is easier,

changes of habits in accordance with new facilities or new dangers have been distinctly recognised. There can be no necessity for us to quote the cases of alterations in the nidification of birds given by Mr. Wallace.* Recent American observations show that the habits of many birds, mammalia, and even fishes, have undergone a very decided alteration in settled districts as compared with less frequented regions. All species have become more wary and circumspect in their movements, and are decidedly more nocturnal. The birds build their nests on higher trees, or in the densest thickets. Any unusual object placed in a river alarms the fishes more than a similar object would have done some years ago and more than it does now in solitary parts of the country. A new danger is recognised, and precautions are taken accordingly.

On carefully examining the habits of ants we find that there exist among closely-allied species, and even in different colonies of one and the same species, gradations which to our mind supply powerful evidence that such habits cannot have been primordial. The slave-making propensity and the reliance placed upon slaves occur in several species, but not to the same degree. *Polyergus rufescens*, for instance, is absolutely dependent upon its slaves, and would without them perish from sheer incompetence to manage its own affairs further than by conducting slave-hunts. It is a military aristocracy, which can fight, but will rather die than work. *Formica sanguinea*, on the other hand, has much fewer slaves, and restricts them to a much narrower sphere of duties, being itself capable of working as well as of fighting. It is curious that the raids of slave-holding ants are confined to worker-pupæ of the species which it subjugates. No instance has reached us of ants carrying off male and female pupæ with a view to raising a stock of slaves in their own city, without the necessity of obtaining them by war. Surely the most rational way of accounting for this slave-making propensity is to suppose that, as in the human race, it is a gradual outcome of war. Ants, in the wars which they are known to wage against different species, as well as against their own, would take prisoners —an undeniable fact—with the original intention of killing and devouring them. Some few of these victims, escaping immediate slaughter, might, if of a docile and submissive disposition, be found useful, and might hence be allowed to live in servitude. Prisoners of fiercer and more indomitable

* Contributions to the Theory of Natural Selection, p. 227.

species, if taken at all, are no doubt killed. The query here naturally arises, What happens in the not infrequent wars between two cities of the same species? Are the prisoners slaughtered, or are they incorporated with the victorious nation?

No less variation may be traced in the habits of the cattle-keeping ants. Of the honey-secreting Aphides and *Cocci* that serve them as milch-kine, some have large herds, some small ones, whilst others have none at all, and if they encounter an *Aphis* straightway kill and eat it. Is it not more probable that the ants first sought Aphides, like other insects, for this very purpose, but gradually discovered a way to turn them to better account than that a flock of Aphides was, by some wonderful coincidence or interposition, placed within the reach of the first ant-hill.

It would therefore, in our opinion, be exceedingly imprudent to declare that ant-civilisation has not advanced, may not now be advancing, and may be destined to take yet further steps in the future, especially if large and fruitful portions of the globe are long allowed to remain in an uncultivated or semi-cultivated state. But such advances must necessarily be slow, as in all cases where there are no means of recording the experience of one generation for the benefit of the succeeding, and where what among mankind would be known as oral intercourse is limited by shortness of life. What direction these future advances may take it is as difficult to indicate as to foretell the discoveries and inventions to be made by man during the next century. But we may safely say that they will not consist in the introduction of tools or weapons, or machinery. Were man, in proportion to his size, about twenty times as strong as he is at present,—were he provided by Nature with a pair of forceps, playing laterally, and capable of being used for felling trees, for excavating the ground, or for cutting off the heads of his enemies, he would scarcely have been a tool-inventing and tool-using animal. A being which, like the Sauba ants of Brazil, can construct a tunnel underneath the bed of a river as wide as the Thames at London Bridge, is in no need of shovels, pickaxes, or barrows.

That ants, in tropical climates, occasion much loss and annoyance to man is indisputable; yet the annihilation of all kinds of ants, were such a measure practicable, would scarcely be prudent. Here, as elsewhere, the rule holds good that small Carnivora are to be cherished, and small Herbivora and Omnivora destroyed. The carnivorous ants, such as the *Ecitons*, are invaluable, from the myriads of

cockroaches, scorpions, centipedes, venomous spiders, grass-hoppers, and even rats and mice that they destroy. They keep down serpents, also, by devouring their eggs. The plant-eaters, on the contrary, and especially the leaf-cutters, are an unalloyed evil, and their destruction ought to be attempted in a much more systematic way than what takes place at present. Nor can the "cattle-keeping" ants be tolerated. Even though they may not in their own persons attack the fruits and the leaves of useful trees, they compass injury to the latter by cherishing and defending swarms of such pernicious vermin as the Aphides of temperate regions and the scale-insects and tree-hoppers of warmer climates. All these live by sucking the juices of plants, and over them the ants watch with a wonderful care—defending them from the attacks of birds, wasps, ichneumons, and other creatures who would rid the poor plant of its parasites. They have even been known to build galleries of clay over the surface of a pine-apple, in order to shelter the *Cocci* who were destroying the fruit.

Mr. Belt found that a red passion-flower, which secretes honey from glands on its young leaves and on the sepals of its flower-buds, was carefully guarded by a certain species of ant (*Pheidole*), who consumed the honey, and who furiously drove off all leaf-cutters and other intruders. But after a couple of seasons a colony of parasitical scale-insects, which secrete honey, established themselves upon the passion-flower, to its great injury. The ants transferred their care and attention to these, and, from the guardians of the plant, became indirectly, but not the less substantially, its enemies. This is a striking proof of the untrustworthy character of our insect—or, more generally speaking, of our animal—allies. At one moment they may be defending our property from depredation, but on a slight change of circumstances their interests may cease to coincide with our own, and they may go over to our enemies. The question what animal-species we ought to protect and which to destroy, and how far we ought to go in each case, becomes on closer inspection exceedingly complicated.

As an example of an omnivorous ant we may take the "fire-ant" of the Amazon, of which Mr. Bates gives us a striking account*:—"Aveyros may be called the head-quarters of the fire-ant, which might be fittingly termed the scourge of this fine river. It is found only on sandy soils, in open places, and seems to thrive more in the neighbourhood

* Naturalist on the River Amazon.

of houses and weedy villages, such as Aveyros: it does not occur at all in the shades of the forest. Aveyros was deserted a few years before my visit on account of this little tormentor, and the inhabitants had only recently returned to their houses, thinking its numbers had decreased. It is a small species, of a shining reddish colour, not greatly differing from the common stinging ant of our own country (*Myrmica rubra*), except that the pain and irritation caused by its sting are much greater. The soil of the whole village is undermined by it; the ground is perforated with the entrances to their subterranean galleries, and a little sandy dome occurs here and there where the insects bring their young to receive warmth near the surface. The houses are overrun with them; they dispute every fragment of food with the inhabitants, and destroy clothing for the sake of the starch. All eatables are obliged to be suspended in baskets from the rafters, and the cords well soaked with copaiba balsam, which is the only means known of preventing them from climbing. They seem to attack persons out of sheer malice: if we stood for a few moments in the street, even at a distance from their nests, we were sure to be overrun with them and severely punished, for the moment an ant touched the flesh he secured himself with his jaws, doubled in his tail, and stung with all his might. When we were seated on chairs in the evenings, in front of the house, to enjoy a chat with our neighbours, we had stools to support our feet, the legs of which, as well as those of the chairs, were well anointed with the balsam. The cords of hammocks were obliged to be smeared in the same way to prevent the ants from paying sleepers a visit." The ravages of the leaf-cutting ant (*Oicodona*), or Saubas of the Brazilians, have been already mentioned. But it also invades houses and carries off articles of food on a far wider scale than is ever done by rats or mice. It is capable of carrying off such a quantity as two bushels of mandioca meal in the course of a single night. Unfortunately the Sauba has few enemies. The number of these depredators who fall a prey to birds, spiders, wasps, tiger-beetles, &c., is too small to be of any importance. The *Pseudomyrma bicolor* easily repels them if they come to clip the leaves of the bull's-horn acacia on which it resides, but it is not sufficiently numerous to pursue and destroy them. The *Ecitons* have never been known to storm the nests of the Sauba. Thus, as we often find, for the greatest mischiefs Nature provides no remedy, and man must step into the breach armed with carbolic acid and corrosive sublimate.

II. THE ATMOSPHERE CONSIDERED IN ITS GEOLOGICAL RELATIONS.

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THE gaseous envelope which surrounds our globe plays a very considerable part in the chemical changes ever going on in rock formations, whether actually at the surface—as in what is called the "weathering" of rocks—or in the less apparent, but perhaps more powerful, action carried on at greater depths whither the atmospheric gases are conveyed by the action of percolating water. It has been shown, by the experiments of Prof. Rogers,* as well as by those of Bischof† and others, that perfectly pure water has a very appreciable solvent effect on rocks and minerals; and that power is immensely augmented, and capability to produce even more momentous alterations in the form of chemical decomposition added, when it is charged with carbonic acid, oxygen, nitric acid, and other matters derived directly or indirectly from the atmosphere.

While, on the one hand, the influence of the atmosphere disintegrates and destroys rock-masses, on the other it is mighty in building them up. Without the small percentage of carbonic acid contained in air—a quantity relatively minute, but in the aggregate enormous—there could be no vegetation. The vegetable kingdom, which obtains its supplies of carbon from those insignificant traces, would be wanting, and there could be none of the coal-beds which form such important members of our rock-formations. This is a direct and palpable case. But if we consider the immense masses of limestones which have been accumulated from those of the Laurentian period, and for aught we know before it, up to the coral reefs of the present day, and which must owe their being indirectly to carbonic acid of former atmospheres, we shall have some idea of the stupendous results attained by very small means, provided time enough be granted.

A drop of rain water absorbs a trace of carbonic acid from the atmosphere, falls on a rock containing lime in some

* Report Brit. Assoc., 1849. Trans. of Sections, p. 40.
† Elements of Chem. Geol., English ed., vol. i. pp. 57, 58.

form, dissolves the lime as bicarbonate, carries it down to the ocean, and finally gives it up to become part of the skeleton of a coral or mollusc, which in its turn may form a portion of an immense mass of limestone rock.

The atmosphere mainly consists of a mechanical mixture of oxygen and nitrogen; these, however, bear to each other an almost constant proportion, any variations being extremely minute. The composition by volume is found to be as follows:—

Oxygen	20·80
Nitrogen	79·20

Carbonic acid,* 3 vols to 10 vols. in 10,000 vols.
Ammonia, a trace; 0·1 to 135 vols. in 1,000,000.
Nitric and sulphuric acids, traces occasionally.

The respective amounts of oxygen and nitrogen do not vary to the extent of as much as 1 per cent, even in exceptional cases. Regnault's analyses of samples of air collected in various parts of the globe gave very close results, the percentage of oxygen being to all intents and purposes identical, viz., 20·9 per cent. Air collected by Sir James Ross in the Arctic Regions did not differ in this respect from that collected at Paris, or at Ecuador in South America; the very slight differences that have been observed not exceeding those noticed in air collected at the same place at different times: and the same results have been obtained from air collected at the summit of Mont Blanc, and even from that taken at a height of 21,000 feet by Gay-Lussac during a balloon ascent. There is therefore a marked uniformity in the aërial mixture under all circumstances.†

It has not yet been explained how it is that a mere mechanical mixture should have this constant composition, but it is certain that the gases are not chemically combined—

1. Because the proportion of the constituents bear no simple relation to the atomic or combining weights of those elements.
2. When they are mixed in the proper quantities there is no contraction, nor is there any evolution of heat, and the mixture acts in every way as air.

* Strictly carbonic anhydride; but I shall use the less scientific but more familiar term in this paper to designate it, in accordance with geological custom as regards this gas. Indeed in its geological relations it may be regarded as a true acid when dissolved in water.

† From some recent observations by Boussingault, and Miller, it would appear the amount of oxygen slightly differs at various heights. Mendeleeff thinks Gay-Lussac's results are probably incorrect (*Bull. Soc. Chim.* [2], xxv., 394). However, we have hardly decisive information yet on this point.

3. Water through which air is passed dissolves the two gases in very different proportions to those in which they are associated, the oxygen being very soluble, while the nitrogen is not taken up to any notable extent.

Carbonic Acid.

Although the bulk of the atmosphere is made up of the two gases just referred to, these do not take so active shares in geological matters as the almost infinitesimal trace of carbonic acid present. This, then, deserves the place of honour in the following pages, and it will be seen that there is a great deal to be said about it. We shall therefore defer the consideration of the behaviour of the other constituents for a little while.

The amount of carbonic acid ranges from about 3 to 10 volumes in 10,000 volumes of air, and the proportion varies between these limits in different localities, owing to many modifying causes. In the neighbourhood of towns or cities it will be much increased by the combustion of fuel, the exhalations of animal life, and the decay of organic matters. In the vicinity of large forests, swamps, and fens, vegetable decay will also augment it, though at the same time the living vegetation there will help to re-absorb it, or, to speak exactly, to decompose it. Near volcanoes the air will be more or less impregnated with it; and from many mineral springs, and subterranean caves and fissures, a very considerable quantity of this gas is discharged into the atmosphere. The percentage of carbonic acid also varies slightly between day and night.

Geological Effects.

So small a trace as even 10 in 10,000—taking the maximum, only 0·1 per cent—certainly does not at first sight seem capable of performing any very great geological work; but we must recollect that the vast quantities of existing vegetation are entirely dependent on the carbon they obtain from the atmosphere, and the decay of vegetation, and consequent liberation of carbonic acid, has a very powerful effect in the alteration or solution of rocks. However, the direct action of atmospheric carbonic acid on rocks—both as a destructive and as a recuperative agent—must be anything but small, even at the present day. As to the latter, it is only necessary to refer to the immense coral reefs now being formed, while the widespread deposits of ooze and mud over the floors of the Atlantic and Pacific are largely

due to carbonic acid entrapped by rain-water and carried down into the ocean. On the one hand, the carbonate of lime previously conveyed by river waters is held in solution, and kept in a fit state for assimilation by marine organisms. On the other, the dead shells while sinking through great depths are attacked, forming, as Sir Wyville Thomson tells us, if the depth is not sufficient to give time for complete decomposition, a calcareous ooze; at greater depths the deep sea muds.* Thus a very great amount of the carbonate of lime in the ocean owes its existence entirely to atmospheric carbonic acid, either from the direct action on calcareous rocks, whether old limestones or silicates,—or indirectly through a series of changes whereby carbonate of soda would be produced, and this being brought into contact with the chloride of lime so abundant in the ocean, carbonate of lime would result. There can be no question but that such effects are going on extensively day by day.

Influence of Vegetation.

If we follow the series of rock-metamorphisms due to the simple absorption of carbonic acid by a plant the result will be seen to be more than interesting. The carbon is assimilated by the plant, an equivalent of oxygen being exhaled. The plant dies, and may become either a part of a coal-bed or may be separately imbedded amongst layers of sediment of some kind. Slow decomposition will now set in, sooner or later, and, if there be a reducible compound near it, chemical changes result. Say the strata contains sulphate of iron: this is reduced to sulphide, commonly known as iron pyrites, a very common mineral in coal-seams—as colliery owners know too well—or in other strata where plants abound. The reduction is effected by the carbon of the plant abstracting the oxygen from the sulphate, and the resulting carbonic acid either is taken up by percolating water, and penetrates farther into the heart of the rock, effecting new changes, and producing carbonates, or it finds its way to the surface through some crevice or by the aid of a mineral spring, and once more mingles with the atmosphere, to be perhaps again absorbed by vegetation, and pass through a round of similar changes afresh. Carbonic

* It now appears, however, that a considerable portion of these muds is derived from the gradual disintegration of pumice and other volcanic *débris* very widely spread over the sea-bottom. See Mr. John Murray's paper on the "Distribution of Volcanic Débris" (Proc. Roy. Soc. Edinb.). The result still due, however, to the action of carbonic acid dissolved in the ocean.

acid exhalations are very abundant at the surface of the earth, and are in great part ascribable to the oxidation or decay of organic matter which in the first instance derived its carbon from the atmosphere.

The above case shows the result of slow decomposition at great depths; but similar effects are induced by the decay of organic matter near or at the surface. In swampy grounds, lagoons, and deltas, such as those of the Mississippi and the Sunderbunds, the decay of organic matter must exercise a very powerful influence on the chemistry of the soils, rocks, and sediments with which the water charged with the compounds formed during the process of rotting comes in contact. Peroxides, such as those of iron and manganese, will be reduced to the proto state, and will be rendered soluble and carried away in solution, to be after a while re-oxidised and deposited in such masses as to be worth working as ores. Silicates of soda, lime, and magnesia will be decomposed, and removed as carbonates; and sulphates, which are usually present in most waters, will be reduced first to sulphides, and eventually decomposed with evolution of sulphuretted hydrogen. Such a process as this may be observed every autumn in the North of Ireland during the maceration of the flax plant, which is placed in pits filled with water, and, being allowed to remain for some weeks, the softer tissues are rotted away, leaving the fibres fit for manufacture. The stench of sulphuretted hydrogen from the decomposing flax is almost unbearable. Having analysed the mud which subsides to the bottom of the flax-pits, I find that the reducing power of the rotting tissues are as described above. The clay in which the pits are sunk contains nearly all the iron present in the ferric condition when not subject to the action of the plants, but in the mud from the bottom there are only proto-compounds, the iron mostly as carbonate. Nor is there a trace of peroxide of iron in the flax-water, but, on the contrary, plenty of ferrous iron.

Clay-Ironstone.—After this fashion must have been formed the clay-ironstones of the coal-measures. The great swampy estuaries of that period may be regarded as gigantic flax-pits; and the rotting vegetation not only altered other salts and compounds of iron to carbonates, but prevented the oxidation of such carbonate of iron as might have been carried down in solution, until in course of time it also was precipitated along with the clayey sediments.

During such changes near the surface a very large

proportion of carbonic acid is returned to the atmosphere. And that there must be, and always has been, this constant circulation of carbon between the earth and the atmosphere is self-evident. What time it originated must be beyond our ken, but so far back as we have any knowledge of there are evidences in the rocks of vegetable or animal life. And the decomposition of such carbonaceous matters, whether at the surface, immediately after death, or whilst buried under a depth of strata,—as in the case of coal-seams,—has always yielded carbonic acid to the atmosphere. At the same time the carbon returned in this way falls far short of what has been abstracted. But, as Bischof points out, the carbon acts as a carrier of oxygen between the mineral kingdom and the air.

Formerly Greater Abundance of Atmospheric Carbonic Acid.

It has long been considered probable that in remote ages the proportion of carbonic acid was greater than it now is, more especially during the Carboniferous period. The remarkable luxuriance of vegetation of a tropical *facies* during that era, in every part of the globe,—even the polar regions,—indicates a very warm climate universally, and it is also thought to imply a much larger supply of carbonic acid than is now noticeable in the atmosphere. The rarity of warm-blooded animals has been pointed to as corroboration of this view; but strictly this is only negative evidence, the absence of fossil forms affording no proof as to the non-existence in bygone time of animals of any particular type. However, a very curious fact bearing on the question has resulted from Prof. Tyndall's researches on radiant heat. It appears that a very small addition of carbonic acid to air renders it absorptive and retentive of radiant heat, and a slight increase in the percentage of carbonic acid in the atmosphere would have a very distinct result. The visible rays of the sun could pass through the atmosphere to the earth; but the radiant heat from the earth, instead of being dissipated into space, would be imprisoned by the atmosphere, which would thus form a warm envelope around the earth, converting it in fact into an immense greenhouse. The glass roof of a conservatory acts in precisely the same way: it permits the solar rays to penetrate freely, but absorbs and cuts off the escape of the radiant heat, and the interior temperature is thereby rendered tropical. Granting, then, the former abundance of carbonic acid, the extreme richness of the carboniferous vegetation, its tropical character and wide

distribution are very fairly accounted for. I shall show presently that there are other grounds for the supposition that the carbonic acid is now much less than it has been in these far back periods; nor is it to be considered that it reached its maximum even in the Carboniferous age. It is true that the earlier formations afford nothing like such a superabundance of fossil plants; but this has been well accounted for by Dr. Sterry Hunt. He has shown that the vast amount of chemical action that has taken place in the reduction and accumulation of the metalliferous deposits of the older Palæozoic rocks will readily account for the scarcity of fossil vegetation in those rocks. To the decay of plants and the reducing action of the resulting carbonic acid those deposits must be in great measure attributed; and their existence proves that an abundant flora flourished. The manner in which this chemical action takes place will be explained further on. I shall just quote Dr. Hunt's words on this point:—"Where are the evidences of the organic material which was required to produce the vast beds of iron-ore found in the ancient crystalline rocks. I answer that the organic matter was, in most cases, entirely consumed in producing these great results, and that it was the large proportion of iron diffused in the soils and waters of these early times which not only rendered possible the accumulation of such great beds of ore, but oxidised and destroyed the organic matters which in later ages appear in coals, lignites, pyroschists, and bitumens. Some of the carbon of these early times is, however, still preserved as graphite, and it would be possible to calculate how much carbonaceous material was consumed in the formation of the great iron-ore beds of the older rocks, and to determine of how much coal or lignite they are the equivalents."*

If we also reflect that the enormous quantities of limestones which are found in the older formations have been largely dependent on the carbonic acid of the atmosphere—in effect, the further we retrograde towards a primitive condition of things the more directly such carbonic acid must have come into requisition for such purposes, as there would be the less of it stored up in rocks, to be re-utilised as at the present day, when much of the carbonate of lime in waters is obtained by the disintegration of pre-existing limestones—and remember also the carbon that was required for the teeming animal life of ancient times, we shall see that there

* "On the Origin of Metalliferous Deposits."—Chem. and Geological Essays, p. 229.

could have been no lack of carbonic acid; and it becomes a matter of small difficulty to accept the theory that a retrogressively greater proportion of carbonic acid gradually leads back to a primitive atmosphere in which that gas—as well as perhaps other gaseous acids, such as hydrochloric acid—was very abundant.

In regard to this question as to the increase or decrease of carbonic acid, a variety of very interesting points suggest themselves, and the facts almost altogether to range themselves on the side of a progressive decrease of carbonic acid. It seems certain that the amount of carbon stored up in the recesses of the earth very far exceeds that of the entire quantity combined as carbonic acid in the air. It is true that Liebig supposed the carbon so combined, which he calculated to reach 2800 billions of pounds, equal to about 1,250,000,000,000 tons,—figures and tons will probably aid in a better conception of this enormous weight,—to be far in excess of all the carbon stored up in coal-beds, and in plants on and in the globe. But this will hardly be subscribed to when we remember that the coal of the British Isles alone, as estimated by the late Coal Commission, is about 195,000,000,000 tons (I have added about a third for waste, &c., deducted in the original estimate). The carbon in this will weigh about 146,000,000,000 tons, taking an average of 80 per cent. But this was only calculated for coals fit for use, of not less than 1 foot thickness, lying at no greater depth than 4000 feet. Now if we include all the coal of inferior quality, of less than 1 foot thick, and at greater depths than 4000 feet, and then throw into the balance the enormous supplies of coal of the rest of the world and of the older and newer formations, not to speak of the highly carbonaceous shales, slates, schists, and clay ironstones, I think—even taking only this branch of the subject—we should rather be led to agree with Bischof, who, on the other hand, calculates that there is at least 6620 times as much carbon in the earth as Liebig has estimated for the atmosphere;* and Bischof's calculation is based on the very moderate assumption that the average proportion of carbon in all rocks is at least 0·1 per cent, which he considers—and no doubt justly—must fall far short of the real amount. This being so, it would certainly appear that there has been more carbon accumulated in the

* Bischof, Chem. Geology, vol. i., p. 204. Dr. Sterry Hunt has also estimated the amount of carbon secreted in the earth as far beyond that contained in the present atmosphere.

earth than has been restored to the atmosphere by decomposition, and that therefore the quantity of carbonic acid in the air has been gradually lessening from remote periods up to the present time. This appears anything but improbable, remembering the arguments already noticed in favour of the supposed highly-carbonated atmosphere of the carboniferous period; and although the calculations leading to such a conclusion are necessarily based on very imperfect data, it may be safely affirmed, at least, that such a state of affairs is not only possible, but probable.

In these calculations we are not only to consider the carbon of the vegetable kingdom, for it will be obvious that any *animal* carbon which may remain in rocks is also more or less directly derived from the carbonic acid of the atmosphere. Taking the extreme case of the *Carnivora*, it is clear that they must ultimately depend on the air for their supplies of flesh-forming material. Say a tiger dines off a cow; the carbon and nitrogen of her flesh have been obtained from vegetation, which in turn extracted them from the air; so that we have a kind of physiological "House that Jack built." "This is the Tiger that ate the Cow that devoured the Grass that absorbed the Carbon," &c. Viewed in this way it seems that "living on air" is a more substantial kind of existence than has usually been supposed.

Now this which is true of the higher animals applies equally with regard to lower forms. There will be a vegetarian somewhere to fall a prey to a carnivorous marauder, who in his turn may be the victim of a stronger individual; and the successive appropriations may go through any number of steps. Thus the carbon and nitrogen of forms of animal life now fossil have been also derived from the atmosphere. We do not find much, if indeed any, of this carbon in its original form now, or directly traceable to animal agency, because highly nitrogenous organic substances decay very rapidly, but it is not unlikely that their results are to be seen in carbonaceous and bituminous shales, and oleiferous rocks such as those in the neighbourhood of petroleum springs; for, as Dr. Sterry Hunt remarks, since animal tissues contain the elements of cellulose, plus water and ammonia, they may give rise to similar hydrocarbonaceous bodies to those derived from vegetable substances.*

In many cases, also, the decomposition of these animal tissues would result in the formation of carbonates, so that on the whole there must be through this source a vast

* Chem. and Geol. Essays. "On Bitumens and Pyroschists," p. 179.

quantity of carbon—originally drawn from the air—locked up in the crust of the earth. And to all must be added the immense amount of carbon combined as carbonate of lime due to the direct solvent action of atmospheric water on calcareous rocks and minerals. If we add all this to the vegetable carbon already considered, there can hardly be a question but that the amount of carbon abstracted from the atmosphere and hidden away in our globe very, very far, exceeds the proportion present in the air of this age. If this be granted—and I cannot see any possible evasion of it—we must admit that the more ancient atmospheres contained far more carbonic acid than that which now envelopes us, and must renounce the doctrine of Uniformity in this connection at any rate.

Origin of Carbonic Acid.

Having got so far, we are naturally led to inquire as to the origin of the carbonic acid in the first instance. Carbon is so thoroughly associated in our minds with organic matter, or in fact with *life*, that it is difficult to conceive the possibility of its existence in an azoic world, and the difficulty is aggravated by the recollection that the earth must have been at the beginning in a state of incandescence, not to go further and say a gaseous condition. However, under the influence of extreme heat, many elements are isolated which at lower degrees of temperature—but still very great—combine and form chemical compounds. For example, hydrogen and oxygen at a high temperature unite to form water, but at a still higher are again dissociated; and we know that hydrogen exists in a state of incandescence, not combustion, in the sun's photosphere.* Similarly free carbon might have been one of the gaseous constituents of the earth in its nebulous phase,† and as the temperature lowered might have been consumed, or united with oxygen, and gone to form part of the primeval atmosphere. In this way all the carbon now in the crust of the earth would necessarily have been at first confined to the atmosphere. Then when rains began to fall, the carbonic acid, being carried down upon the earth, would soon decompose the silicates which must have resulted from the cooling down of the original heated mass; carbonates would be formed and carried down into the primitive oceans, and clayey residues would be left behind.

* Prof. Henry Draper has just announced the discovery of oxygen in the sun. Nature, August 30, 1877.

† According to Mr. J. Lawrence Smith, carbon in the gaseous form is spectroscopically manifest in the attenuated matter of comets. Am. Journ. Sci., June, 1876.

In course of time, when vegetable and animal life had made their *début*, the withdrawal of the carbonic acid from the air must have proceeded much more rapidly, and the atmosphere gradually cleared to such a condition as to permit of the existence of air-breathing animals. It may be here remarked that the very gradual introduction, in more recent periods, of warm-blooded beings, would also coincide with the hypothesis of the originally highly mephitic state of the atmosphere.

Carbonic Acid now Increasing or Decreasing?

An important question now arises—Is the amount of carbonic acid increasing or decreasing, and what may the result be in either case? To begin with the last part of the question :—Any considerable difference one way or the other must result in a diminution of animal life; in its higher forms in the former event, in all divisions in the latter. Beyond a certain proportion very little above the ordinary standard—at most 10 times, equal to about 5 vols. in 1000,* or 0·5 per cent!—carbonic acid in air becomes a deadly poison to all warm-blooded animals. On the other hand, a diminution in the percentage of carbonic acid would tell even more severely. Vegetable life would languish, graminivorous animals would eventually have nothing to eat, and, finally, the Carnivora, being obliged to prey upon each other, would of course become extinct. And this would be applicable to all divisions of the animal kingdom. The result would be a completely barren and desolate planet, perhaps in some degree resembling the moon. Doubtless that planet has passed through phases of existence alike to those which have obtained upon the earth; and Mr. Proctor† is of opinion that the moon certainly had originally an atmosphere, which is now either altogether absent or is attenuated to an extreme degree. It can well be imagined that this result, and its consequent azoic addition, has been brought about by some such absorption of the constituents of the moon's atmosphere as that which I have endeavoured to sketch out above as regards the earth.

Probable Withdrawal of Oxygen.—It may seem a little paradoxical that such dire effects would more immediately follow the withdrawal of a poisonous gas, and that the latter is on the whole more important to the continuance of life than oxygen gas, which is almost inseparable from our ideas

* Watts, Chem. Dict., 1862, vol. i., p. 438.
† Quart. Journ. Science, July, 1874. "On the Past History of our Moon."

of existence; but it is undeniable that such would be the case. The blood requires to be oxygenated, but in the absence of carbon there would be no blood at all. And this leads us to another point. The disappearance of carbonic acid must be followed after a period by the withdrawal of oxygen itself. It would gradually be carried by water into the interior of the earth, from which it could make no return, for it would be seized upon by compounds capable of oxidation, and its retreat in the form of carbonic acid would have been cut off.

As to the first part of the question, however, we have as yet no data for its solution. There are several means by which carbonic acid is supplied to the air, and many by which it is removed; but we are not in a position to determine on which side is the predominance, or whether there is at present a balance of power. The principal sources of increase are—

1. Volcanic and other subterranean exhalations.
2. Respiration of animals.
3. Combustion of fuel, &c.

Respecting this last it should be pointed out that we are now restoring to the atmosphere some of the vast quantities of carbonic acid abstracted from it during the Carboniferous period, and imprisoned for ages in the interior of the earth in the forms of coal and clay-ironstone. Perchance by the time we have made an end of our supplies of coal a very sensible difference will have been effected in our atmosphere.

The absorption of the carbonic acid is brought about thus:—

1. By vegetation, as already explained.
2. By the agency of marine organisms which secrete carbonate of lime.
3. By the direct action of atmospheric carbonic acid upon rocks, resulting in the formation of carbonates.

How far these antagonistic processes check each other cannot be conjectured. In order to arrive at any conclusion on the matter we should require to compare trustworthy analyses of air taken at frequent intervals during some thousands of years at least. We have yet no recorded analyses of it older than forty or fifty years. Probably in the remote future information will have been accumulated sufficiently to allow of the solution of the problem; and perhaps in those far distant times a Royal Commission, or some such form of Public Inquiry, will be solemnly convened

to deliberate as to the possible duration of "Our Carbonic Acid Supplies." But should a necessity ever arise, it is comforting to reflect that it is not likely to occur until some ages after the travelled New Zealander has been gathered to his fathers, and even the very sites of Auckland and Otago perhaps long a subject of curious speculation amongst Central African *savants*. I say it is comforting to take this to heart in these days of sensational cosmogony, when one day we are threatened with destruction from the sweep of a comet's tail, and the next an unfavourable eruption of sun-spots may entail unheard-of miseries upon us. All the information we are in possession of goes to show that the trifling changes that are now observed in the condition of the atmosphere would perhaps require a continuance throughout many millions of years before making themselves disagreeably apparent.

Geological Influence of Oxygen.

This comes next in importance as a geological agent.* I have dwelt first upon the results wrought by the carbonic acid, because the work done by it is immensely greater in proportion to its amount. But oxygen also has its mission. Percolating the rocks, dissolved in rain-water, which is able to absorb a very large quantity of it, it quickly reacts on all oxidisable substances. Carbonates and proto-salts are converted to peroxides; sulphides are changed into sulphates, and sometimes this is accompanied by the production of double salts, such as alums. A familiar instance may be referred to as occurring in the spoil banks of coal-pits, where quantities of aluminous shales, with refuse coal containing iron pyrites, are heaped up together and exposed to the influence of the weather. The oxidation of the iron pyrites results in sulphate of iron, and the sulphuric acid so formed —reacting on the alumina, potash, &c., of the shales—forms a more or less complex alum, which may be observed in small stellate crystals between the laminæ of the shales. Alum slates and earths are very common, and all owe their origin to the oxidation of iron pyrites, or some other sulphide, under circumstances akin to the above.

Ores and Metalliferous Deposits.

The peroxides of iron and manganese are of considerable importance, both commercially and from a scientific point of view. In many cases their formation may be traced

* The amount of oxygen in the atmosphere is about two trillions of pounds (Bischof, *op. cit.*, i., 204), equal to about 892,857,000,000,000 tons.

directly to the action of atmospheric oxygen. In other instances this action is but veiled by a series of complications. Many valuable deposits of iron and manganese are formed in cavities of rocks through the means of water containing carbonic acid and oxygen. The first dissolves the minerals as bicarbonates; then, the excess of carbonic acid escaping as opportunity permits in open fissures, they are oxidised, and deposited at once in an insoluble form, while such other carbonates as happen to be in solution, and which—like lime, magnesia, and the alkalies—have a stronger affinity for carbonic acid than for oxygen, are carried away.

By such a process as this immense beds of limonite have been deposited, and the liberated carbonic acid restored to the atmosphere. Bog iron-ores and the well-known lake iron-ore deposits of Sweden are cases in point. Some of these deposits are assisted by organic agency, some of the Diatomaceæ—*Gallionella* in particular—being very active in this way; but they are only accessory aids, the real work being due to chemical reactions between carbonic acid, oxygen, and soils or rocks. The extensive beds of hematite associated with the Antrim basalts are unquestionably lake-deposits, as Prof. Hull has suggested,* and must be due also to the reciprocal chemical action of the carbonic acid and oxygen from the atmosphere. These beds are now intercalated between the sheets of basalt, and sometimes reach a considerable thickness, consisting of beds of rich ore, poorer ore, and "lithomarge," which is a highly ferruginous clay. Prof. Hull considers that all these were deposited in a large lake or series of lakes. Assuming this, the *modus operandi* was probably this:—The highly ferruginous basalt forming the shores of these lakes being subject to the action of atmospheric water, the iron existing as proto-silicate in the augitic rock, was dissolved out as carbonate and carried into the lake. The excess of carbonic acid then escaping, oxidation ensued, as in the case already referred to, and the iron was precipitated as a hydrated peroxide. At the same time fine sedimentary aluminous matter was also carried down and deposited, and, according as the amount of this was greater or less, a bed of lithomarge or workable ore was laid down. A fresh volcanic outburst eventually taking place, the lakes were covered in, and the ore bed preserved from denudation.

The ore must have been precipitated in the hydrated state,

* Brit. Assoc. Report, Belfast, 1874; also Ex. Mem., Sheets 21, 28, 29 Geol. Surv. Ireland.

and the water of combination was doubtless afterwards given off spontaneously, in the same way as by hydrate of alumina and the hydrated forms of silica. There is indeed considerable analogy between the hematites and the colloid forms of quartz. It is only necessary to compare these pisolitic and botryoidal iron-ores with the calcedonys to see this, and the comparison would be in favour of the aqueous origin of such iron-ores were fresh proof needed.

It will be obvious that the reactions sketched out above with regard to iron ores and compounds applies equally to all other minerals capable of being oxidised or reduced. Copper pyrites, for instance, is often oxidised to sulphate, and the carbonate altered to oxide just in the same manner.

Antagonistic Action of Carbonic Acid and Oxygen.

Clearly, then, the carbon and oxygen derived from the atmosphere sustain antagonistic parts in their action on rocks and minerals. They are perpetually warring the one against the other, and thus keeping up a circulation between the earth and the air. The carbon reduces the oxides whenever it encounters them, and the oxygen replaces the carbonic acid of carbonates with the same inveteracy. The combined effects of these elements in geological transformations is extraordinary when we come to reflect on it. Regarded from an utilitarian point of view, to them we owe probably every metalliferous deposit of value in the world. I have shown how a highly ferruginous rock, such as basalt, containing proto-salts of iron, which are soluble in carbonic acid, might be acted on directly by that acid from the atmosphere. But there are cases where insoluble compounds of iron in small quantity, locked up in rocks, are, by the reducing action of the carbon of decaying vegetation, liberated, and finally accumulated in such quantities as to be of commercial value. Soils and clays contain small portions of peroxide of iron, which is insoluble. The decay of vegetation or other organic matter robs this of oxygen, giving rise to carbonic acid. The resulting *protoxide* is soluble in water containing carbonic acid, or other organic acids, and is carried down into lakes or fissures, where, again absorbing oxygen, it forms beds or veins of hematite.

While insoluble oxides are rendered soluble and allowed to accumulate in this way, soluble sulphates are reduced to insoluble sulphides,—iron pyrites, copper pyrites, zinc blende, galena, &c.,—and, as Sterry Hunt puts it, "removed from the terrestrial circulation," for a time at least. Such

are the processes to which many metalliferous deposits are due.

Another result of the opposition of these two atmospheric gases is the defertilising of soils, and consequent failure of vegetation. An ordinarily fertile natural soil contains, amongst other things, silicates of alumina, lime, potash, and soda, with some peroxide of iron. The silicates of lime and soda will be decomposed by carbonic acid, and the bases removed as carbonates. The potash silicate is also decomposed, and a part of the potash removed by aquatic plants under favourable circumstances, in marshy places, &c.,—conditions under which the vegetation of the Coal era flourished,—and the ferric oxide is reduced to the ferrous state by the deoxidising influence of rotting vegetation. This having occurred, the roots of plants are for a time debarred from any access of oxygen, for any that permeates the soil will be immediately seized on by as much of the proto-compound of iron as has not been carried off in its soluble state, and this is again converted to the higher condition; and these changes continue until they result in the total barrenness of the soil and its ultimate conversion into a hydrous silicate of alumina, almost entirely free from iron, such as we are acquainted with in the fire-clays of the coal-measures—those ancient soils on which the vegetation now forming our coal-seams once grew.*

Ammonia and its Compounds.

Ammonia exists in the air chiefly in the form of carbonate of ammonia, but the quantity, whilst always small, appears to vary greatly, and it is not positively ascertained whether the variation is to be ascribed to natural causes, or ought to be referred to the difficulty of accurate analysis when such small quantities have to be dealt with. It is quite possible, however, that the variability is natural. The minimum recorded is 0·1 part of carbonate of ammonium in one million of air; the maximum is 135 parts.† Rain-water, hail, snow, and dew contain appreciable quantities of ammoniacal

* It is obvious that this only applies to natural soils, since the agriculturist by breaking up the ground affords a supply of oxygen much in excess of what is absorbed by the oxidisable matters present.

† Watts, Chem. Dict., p. 439. P. Truchot finds that the amount of ammonia varies with the altitude. At Clermont-Ferrand, 395 metres above sea-level, the quantities were 0·93 m.grm. to 2·79 m.grms. in a cubic metre of air,—according as the day was clear or dull,—whilst at Pic de Sancy, 1884 metres, it amounted to 5·27 and 5·55 m.grms. under the same conditions. Comptes Rendus, lxxvii., 1159—1161.

salts, and in rain from thunder-showers the ammonia is combined as nitrate, the effect of the electric discharge being to oxidise a portion of the nitrogen of the air to nitric acid.*

The atmospheric ammonia is not without its effect on vegetation. It is certain that plants grown in air perfectly free from ammonia never flourish to the same extent as those surrounded by an atmosphere containing some of it; and the experiments of Boussingault, Lawes, and Gilbert—borne out as they are by those of Stöckhart, Peters, and Sachs, and lately by the very conclusive researches of Schlœsing† and A. Mayer‡—show that at least a considerable part of, if not all, the nitrogen of plants is derived from this source. Now the geological connection of this is at once plain, for the decomposition of nitrogenous matter such as plants, in rocks, may lead partly to the formation of nitrates, or, by the evolution of nitrogen and ammonia in volcanic regions, give rise to other minerals, as I shall show presently.

Occasionally the ammonia is absorbed directly from the air by surface mineral matter, as in the case of the volcanic earth of the Solfatara of Puzzuoli. S. de Luca‖ tells us that this contains a quantity of sulphur and arsenic which under the influence of air and moisture form acids, and at once, combine with the atmospheric ammonia. But it is to the decay of vegetation that the vast majority of the nitrogen compounds which are met with, either as minerals or as volcanic emanations, are due, and in whatever state the nitrogen was originally absorbed—whether in the free state or as ammonia—it cannot be doubted that all the nitrogen compounds contained in the earth, as it now exists, are traceable entirely to past and present atmospheres.

The nitrogenous compounds so obtained are themselves subject to an endless variety of changes, in which the gases already described bear no unimportant parts—reducing and oxidising; and these changes, or the effect of heat, may

* Liebig found that of seventy-seven specimens of rain-water, seventeen, collected during thunder-storms, contained nitric acid combined with lime and ammonia. Of the remaining sixty but two contained traces of it.—Bischof, *op. cit.*, i., p. 214. According to Böttger, the induction-spark passed through moist air gives nitrogen peroxide and ozone, but in dry air gives nitrous fumes. —*Chem. Centr.* (1873), 497. Doubtless similar results follow discharges of natural electricity.

† Comptes Rendus, lxxviii., 1700.

‡ Deut. Chem. Ges, Ber., vi., 1404—1413, and Landw. Versuchs. Stat., xvii., 329.

‖ Comptes Rendus, lxxx., 674.

result in a renewed evolution of ammonia to the atmosphere.

Under such circumstances occasionally the ammonia, instead of escaping freely into the air, meets with hydrochloric as in the depths of volcanoes, and combining with it is evolved as chloride of ammonium (*sal-ammoniac*), which is condensed on meeting with the cooler external air. This mineral is often met with in large quantity, so much so, indeed, as to be of commercial value. Thus during the eruption of Vesuvius in 1794 great quantities of this salt were evolved, and it was collected by the peasantry; and Hecla in 1845 yielded very profitable supplies of it. In the vapours of the Solfatara, at Puzzuoli, it is also met with, and it is found mixed with sulphur and other matters in the crater of Vulcano, where it is now being largely collected,* and in considerable quantity at Etna. Then the volcanoes of Kutsché and Turfan, in Central Asia, afford such large supplies that it has been a very valuable article of commerce.†

Prof. Judd is at a loss to explain the production of those large quantities of sal-ammoniac, unless on Daubeny's supposition that nitrogen under the influence of heat is unusually active; but the matter is readily accounted for thus:—The decomposition of nitrogenous organic matter at all times produces ammonia, but especially so under the influence of heat (a familiar instance is the manufacture of coal-gas). That a sufficiency of such organic matter exists in the rocks through which these volcanoes have burst is undoubted, and the ammonia evolved combines with avidity with the hydrochloric acid‡ also given out in volcanic emanations.

Quite lately a new mineral has been discovered incrusting the recent lavas both of Etna and Vesuvius. This is a nitride of iron named "*Siderazote*" by its discoverer, Silvestri,‖ who considers it is due to the decomposition of ammonium chloride by heat in the presence of ferruginous lavas; and although we may not quite accept his theory that the ammonium chloride is formed by the absorption of nitrogen direct from the atmosphere by the lava, it is certain

* J. W. Judd, "On Volcanoes," Geol. Mag., Dec. 2, vol. ii., p. 113.

† Bischof, *op. cit.*, i., 212—213.

‡ The formation of white fumes of ammonium chloride when a glass rod dipped in ammonia is brought near hydrochloric acid will occur to chemical readers.

‖ "The Occurrence of Nitride of Iron amongst the Fumarole Products of Etna, and its Artificial Preparation." Orazio Silvestri, Gazetta Chim. Ital., v., 301—307. Pogg. Ann., clvii., 165—172.

that the nitrogen has been originally drawn from that source. We may fitly conclude this part of the subject with the mention of the native sulphate of ammonium, *Mascagnine*, of which it may be said that every constituent could have been obtained from the atmosphere.

Nitrogen.

It is obvious that much of what has been said regarding ammonia will apply to nitrogen, but on the whole the latter in its free state appears to have but little influence as a geological agent.

Sulphuric and Sulphurous Acids.

The exceedingly minute traces of these acids make but a slight effect on rocks when compared with the gases already touched upon. That they are not altogether inert may be taken for granted, but both their absorption and re-evolution are of a local nature, being chiefly apparent in the neighbourhood of large towns and about volcanic regions. They may be "withdrawn from circulation" as sulphates and sulphides, and be returned in their original shape, or decomposed into sulphur or sulphuretted hydrogen.

Variations of Atmospheric Pressure.

These cannot but have an appreciable effect on certain classes of geological phenomena. The emanations of gases from the interior of the earth are influenced in some degree. It is well known that explosions in coal-mines sometimes follow a sudden fall of the barometer, which can be well understood on comparing the pressure corresponding to different barometric heights.

Barometer at	28	inches.	Atmospheric pressure	13·70 lbs.
,,	29	,,	,, ,,	14·19 ,,
,,	30	,,	,, ,,	14·68 ,,
,,	31	,,	,, ,,	15·17 ,,

It is usual to refer to the atmospheric pressure as about 15 pounds on the square inch, but the above table shows that a considerable variation makes itself felt within the barometrical range. This must not only control evolution of gases from coal-seams, but also exhalations from open grottoes and caves, mineral springs both thermal and otherwise, and probably from intermittent active volcanoes, such as Stromboli, where the periodical explosion of gases is an

important phenomenon. With regard to this Mr. Judd says "that the barometrical condition of the atmosphere must exercise a powerful influence on such a series of operations as are seen to be going on within the crater of Stromboli, few, probably, would be bold enough to deny." It appears "that the more violent states of activity . . . . coincide with the winter seasons and stormy weather, and its periods of comparative repose occur during the calms of summer, is established not only by the universal testimony of the inhabitants, but . . . . by the actual observations of many competent authorities."* It is hardly necessary to point out that during stormy and wintry weather the barometer is mostly low, while the contrary is the case during summer time and calms.

It is not impossible that similar antagonism between outward and inward pressure may affect the working of many other vents, such as the Solfatara of Naples, and mud-volcanoes, such as those of Sicily, Transylvania, &c.; and that such variations may have no inconsiderable results, both as regards the chemical and cosmical effects of volcanic action.

And now, reviewing the preceding notes, it will be seen what an all-powerful geological agent the atmosphere we breathe is. Without its aid we should know never a stratified formation. The earth would simply form a ball of truly primitive rock, resulting from the cooling down of the original nebulous mass set apart for our globe, the only variation in which primeval and perennial crust being that of the different strata of higher specific gravity towards the interior. We should have no coal, no metalliferous deposits, no rivers or seas, and no rain,—consequently no denudation by "Rain and Rivers,"—for the vapour of water could not ascend into empty space. We should have —— but, last and worst of all, there would be no "we." Life would be impossible, and the earth would finally degenerate into a

—— "pale-faced moon."

That this is probably her ultimate mission cannot be denied. The only consolation is that owing to her larger size, and therefore slower rate of cooling than the moon, she will have gone through a somewhat more extended geological course. There is undoubtedly a very intimate connection between secular cooling and withdrawal of atmosphere, for

* J. W. Judd, *op. cit.*, p. 213.

the cooler the interior the smaller will be the return of gaseous elements to the surfaces; and probably before Saturn and Jupiter have cooled down to a habitable temperature, the senescent earth will roll through space—cold, void, and airless. Sooner or later nothing is more certain than that

—— "to this favour she must come."

III. ON SCIENTIFIC METHOD.*

By M. M. Pattison Muir, F.R.S.E.

WHETHER we turn our attention on ourselves, or seek to pursue the study of mankind in general, or, on the other hand, confine our view to the natural world around us, there is in each case one method by pursuing which we arrive at exact knowledge: that method is the Scientific. What, then, is Science, and what the scientific method? The question, What is Science? is synonymous with another, What is Knowledge? Here is a stone: how do I know it to be a stone? Because it is *like* so many other things which I call stones; it is hard, it possesses a certain colour, it is not easily broken, and so on. I know that it is a stone because I recognise in it certain qualities which I have grouped together and regarded as characteristic of those pieces of matter, to all of which I therefore apply one general name, viz., *stone*. In stones, therefore, there is some quality, or qualities, possessed by all in common, such qualities being sufficient to mark off the possessors of them from all other kinds of matter. Yet these stones may differ from one another in many other ways.

Such a classification is a scientific one. I *know* something about these stones. Were there only one piece of stone in the world I could never know anything about it as a stone. To know we must compare; and the scientific method consists in finding unity amid variety, in tracing the inner relationships between seemingly diverse things (or

* In this paper I have freely availed myself of the stores of information and of the wonderfully original suggestions contained in that most remarkable work "The Principles of Science," by Prof. Jevons, F.R.S.

thoughts), in finding the common link which binds together those things at first sight appearing so widely separated.

This, then is the aim of Science—to know. And to attain this aim she must find the agreements and differences between all things; in other words, she must classify. When we have arrived at a complete system of classification of all phenomena we shall have attained the purely scientific aim of our intellectual existence.

To awaken consciousness there must be more than one phenomenon. Object is compared and contrasted with object, and hereby resemblances and differences are discovered: these are retained in the memory, and compared with other resemblances and differences, as these may be discovered, until at last we are able to find identity amid diversity, to group together a number of objects by means of some great common property, and from this identity to draw inferences which rest on some point of resemblance, and which have for their basis the law that "that which is true of one thing is true of its equivalent." We gradually leave behind us the old idea that ceaseless change is the order of all things; we learn to believe that what we to-day know as Iron will yet be Iron a thousand years hence; we get something definite to reason on, and, step by step, the varied and strange phenomena of Nature are found to be lawful phenomena—are found to have a fixed basis underlying them; until at last we arrive at a general expression for so many and so varied phenomena that we give it the name of a *law of Nature*. Thus we rise from the trivial to the abiding, from the changing to the changeless, from the passing to that which endures, and from that which was capricious to that which is governed by law.

But even here, even in these *natural laws*, we have not attained absolute certainty; they are but general expressions, including a vast number of else isolated phenomena. But what is beyond these phenomena? What is the cause of all these causes? Science, strictly so-called, gives no reply.

It may be urged that modern science teaches that all things are in a continual state of change; that there is no such thing as rest in the physical universe; that every form of energy is but the expression of a change of material particles: but Science, we answer, has gained this knowledge only by grasping the changeless facts underlying the changing phenomena. We do not now simply know that the material universe is constantly undergoing change: we are able, to some extent, to follow the steps of this change; we have reduced the very mutability of Nature to law; we have compared change with change, and in some instances

have succeeded in detecting the connecting-link. We have made a beginning in the classification of the changes of Nature.

If the aim of Science be to detect identity amid variety, it is asked—What means does she employ for accomplishing this end? *Observation, Experiment, and Inference.*

From repeated observations we discover an identity; from a number of identities we infer that what is true of one is true of another; from a number of combined inferences we draw a wider inference, which again we generalise into what is called a law. But before we can establish a law we must make use of the process of deduction—if such or such an inference be true, then this or that phenomenon must follow. Observation or experiment tells us whether the phenomenon does occur or not; if it does, another proof of the correctness of the law is gained; if it does not, there is the less probability that the so-called law is a true one. Thus it is from a series of partial hypotheses which generalise a number of facts that we at last ascend to *the* hypothesis which shall include in its expression all the isolated facts—that is, to a general law. And this method of combined observation, experiment, induction, and deduction is the Scientific Method.

There is nothing pecular in this method; it is but common sense reduced to rule. We are continually and unconsciously guided by the scientific method in our every-day conduct. The countryman who, in the morning, assures his neighbour that it will rain before midday, bases his assurance upon a train of scientific reasoning; he has repeatedly observed that certain appearances of sky, a certain direction of wind, and rain, are associated together: from these observations he has, probably unconsciously, framed the hypothesis that the three sets of phenomena are related together in such a manner that, given the two first, the third is sure to follow; he has proved the value of his hypothesis again and again, by acting on it,—the only scientific method of proving an hypothesis,—and he at last has come to regard it as a law of Nature. But after all it is only an hypothesis, probably a very partial one, and Nature will very likely some day teach him that he has been too ready to narrow her working to the sphere of his own capacities.

The scientific method is applicable, more or less, to all branches of phenomena coming within the scope of human understanding. But the domain claimed by Science is so great as to make it impossible for any man, or body of men, to examine it with completeness. Hence arises the necessity for divisions and subdivisions in Science. The first part which

must be examined is clearly, the laws which regulate reason itself, the laws of thought, and their application to the processes of inference. The study of these belongs to the science of Logic. The application of these principles to reasoning about numbers and quantities constitutes the science of Mathematics, and so on.

We have one set of sciences dealing with physical or material objects; another with mental, moral, or immaterial objects; or with these as they are modified by social relationships. Hence arise the two broad classes of natural or material, and mental or moral science. The scientific method is applicable to both alike, only the questions arising under the second division are more complicated and much more difficult of solution than those under the first. Indeed it is only within recent years that the scientific method has been, in any great degree, applied to the questions of mental and social phenomena.

In endeavouring to classify scientifically the phenomena of Nature we make use of the method, first of all, of Observation.

From experience we gather together a number of facts, but in order to classify these facts we need often have recourse to Experiment.

In the first-named process we do not alter the conditions under which phenomena occur in Nature; we *merely observe* these phenomena as they are presented to us, or at most we *vary our point of view*. In carrying out an experiment, on the other hand, we must carefully vary the *conditions of the phenomenon*, and endeavour, as far as possible, to exclude those which have no influence upon the fact we are studying.

Observation and experiment are the first steps in the ladder leading upward to scientific knowledge.

But where are observations to begin? In our world facts are so numerous, phenomena so almost infinite in number, that no man can say which are to be observed and which neglected. Hence we find that many of the greatest scientific discoveries have taken their rise from what we call "chance" observations. But that which is passed by, by one man, as altogether unimportant, in the hands of another leads to the most important results. The twitching of the leg of a dead frog when accidentally touched by the wires coming from a battery caught the eye of Galvani, and he, following up this chance observation, so added new facts and new theories to our scientific stock that one of the greatest branches of Electric Science now derives its name from this man.

It is impossible, however, to carry out the method of

observation beyond certain limits. Our powers of hearing are not delicate enough to perceive vibrations exceeding somewhere about 38,000 per second; hence if there be sound-producing vibrations quicker than these no amount of observation will enable us to detect them. Not only is this method unable to pass beyond somewhat narrow limits,—it is also liable to lead us to untrue conclusions, unless it be very carefully used. Do we not often hear it said—"See how the buildings of our great ancestors have lasted during the centuries,—strong and firm these old temples, or walls, or roads remain to this day, while the structure which we have raised to-day by to-morrow begins to decay"? Observation tells us that the older buildings remain; observation tells us that the newer quickly disappear; but observation does not tell us that it is only the great buildings of antiquity which remain—the buildings which, from their purpose and design, we should expect to be *very* strongly put together; and that the ordinary houses and the common buildings have all *long ago* utterly disappeared. The conclusion drawn from observation alone—viz., that our ancestors built more strongly than we do—is therefore a conclusion which is not proved by the evidence adduced.

Again, the mind of the observer may be so overcast with prejudice or fancy, or may be so dim and dull as either not to receive aright the image of outward things, or to transpose that image so that it becomes a caricature, not a truthful picture. He who when shown, in the old heathen temple, the picture of all those who had been saved from shipwreck after paying their vows, and asked to believe *now* in the power of the gods, replied "But where are they who paid their vows and were not saved from shipwreck?" was a man whom we have often need to copy.

By *observation alone* we cannot tell the *exact* conditions regulating the occurrence of any phenomenon; these conditions can be determined only by *experiment*. But where an observation has been made, and we are wishful to determine the exact conditions which regulate the occurrence of the observed fact, we shall find that it is very difficult to fix on these conditions. Every fact is so closely related to so many others that it becomes very hard to strike out those conditions which are really non-essential. Nay, it often happens that what at first sight appears to be the most important condition of a phenomenon proves, after experiment, to have little or no influence on this phenomenon while other overlooked circumstances are the true governing causes. Thus if we let fall a piece of lead and a sheet of

paper of the same weight, from a height, we find that the lead reaches the ground long before the paper does. We should naturally conclude—as all, or almost all men did before the time of Galileo—that the *nature* of the two bodies influences the velocity of their descent; whereas it is actually found, by carefully conducted experiments, that no property of bodies except their absolute mass has any influence upon their gravitating powers. In experiment we must therefore seek to eliminate, one by one, those circumstances which are really not of importance as influencing the phenomenon in question; we must simplify the experiment as far as possible, taking care, however, that in our attempts at simplification we do not overlook *the* circumstance really governing the phenomenon. After all, however, some overlooked condition may be present, the non-observance of which entirely vitiates our results. Experiments were long ago very carefully carried out with a view to prove that earth can be formed from water: water was again and again distilled from a perfectly clean glass vessel, yet there remained a small quantity of a white earth in the vessel in which the water was distilled. If the water has not been converted into the white earth, where has this substance come from? The conclusion seemed inevitable, and the conclusion was therefore adopted —water may be converted into earth. But the overlooked circumstance was this:—Water acts on glass, especially at high temperatures, so as to dissolve part of it, and the white earth is really a portion of the glass vessel dissolved by the boiling water, and left in the vessel when the water has been entirely boiled away.

An exceedingly instructive example of the process of elimination of non-important conditions of experiment is afforded by Sir Humphrey Davy's researches upon the electrolysis of water. When water was decomposed by the electric current, an acid and an alkali invariably made their appearance at the poles along with the oxygen and hydrogen. Electricity, some people supposed, caused the production of the acid and the alkali; others imagined that water always contains acid and alkali. By using agate or gold vessels in place of glass, to contain the water, Davy showed that less acid and less alkali was produced. Finally, by carrying out the decomposition in gold vessels, in the exhausted bell-jar of an air-pump, he was able to obtain from pure water, oxygen and hydrogen free from both acid and alkali, thus showing that the presence of air was in some way the cause of the production of the acid and of the alkali.

If, therefore, an experiment seems to point irresistibly to

this or that conclusion, we must be very chary in accepting this result until we have again and again varied the conditions of the experiment so as to bring under notice every circumstance which can in any way influence the phenomenon we are investigating.

To discover what condition may or may not influence a given phenomenon becomes therefore one of the most important problems of the scientific investigator. And here the man of a keen insight and quick apprehension has a very great—in fact, an immeasurable—advantage over the ordinary dull and plodding experimenter.

There seems to be a somewhat widespread idea that there is no longer any use for genius; that magnificent laboratories, elaborate organisation, Government endowment, and certificated teachers are to carry all before them, and achieve results such as the world has never seen. To me it seems that now, as ever, genius is necessary for any really great discoveries; that no amount of training, nor of organisation, nor of artificial selection, can make up for the absence of native talent—of that subtle, scarce definable something, which we call genius.

If the genius is there, by all means let us educate it as best we may; let us also do our utmost to train all, whether possessed of genius or not,—for in doing this we shall, at any rate, be giving to all the means of leading a nobler and a more useful life than they otherwise could; but let us beware of thinking that *we* can evoke this rare and wonderful product, genius, by any method of selection, or by any system of competitive examination: plodding, persevering, patient work is of the very utmost importance; but the power of grasping the one true condition of a problem, to the exclusion of the many trivial but *seemingly* important conditions, is of yet more importance; while he who combines both of these qualities,—he who has genius to see and patience to follow,—he it is who stands forth as the great discoverer, as the poet of Science. The successful investigator of Nature must be patient; he must very often reserve his judgment until experiment proves to him that this or that conclusion must be the right one; he must be ready to frame hypotheses, but he must not shrink from submitting these to the most rigorous experimental test, and when he finds experiment and hypothesis opposed he must be ready to doubt the latter, but yet not despair of finding another truer generalisation; he must never disdain help, even from the humblest; he must have no envy; he must neglect no objection; he must not choose and *then* compare, but after

comparing many times he must choose; and while he is thus humble he must not hesitate to frame hypotheses,—he must risk something in his search for truth, knowing that in rigorous experiment he has a means of trying his queries whether they be true or not. "The philosopher," says Faraday, "should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biassed by appearances; have no favourite hypothesis; be of no school; and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the temple of Nature."

I have said that we still need men of genius who can see through the tangled web of facts and catch a glimpse of the governing power behind. But it may be asked, Is not this a different method from that of strictly inductive reasoning recommended by the great father of true logic, Bacon himself? Yes, it is somewhat different; yet I think that a due attention to historical facts will show us that without deductive reasoning no true generalisations have ever been reached in Science.

By an examination of facts alone we gain empirical knowledge; scientific unity can only be gained by embracing these facts within one general principle. A mere collection of empirical facts does not constitute scientific knowledge; we must explain these facts,—that is, we must take out the folds, "*Ex plicis plana redere*," we must show the resemblances, more or less deep, between the facts; so long as a fact remains alone, unattached and seemingly unattachable to any other, we feel a certain uneasiness, unsatisfiedness, in regarding this fact; and such an uneasiness *may have*—in certain ages *has*—developed into a superstitious dread of the unexplained fact. In other ages than the present the sweep of the comet across the sky was regarded as an omen of evil,—it was an awful unexplained fact,—but now that we know that the laws governing the movements of the comets are the same as those which rule the calm and peaceful stars, we no longer experience any dread at the approach of these, once fearful, visitors. But the man of Science is often taunted with his lack of awe and reverence of the mysteries of Nature: the accusation is, I believe, only made—if made in earnest—by those who cannot take the trouble of investigating Nature for themselves; by those who think it a grander thing to speak of mystery, and greatness, and reverence, than to exhibit those qualities in themselves which

they demand in others. To quote the words of Charles Kingsley:—"There is a scientific reverence, a reverence of courage, which is surely one of the highest forms of reverence, that, namely, which so reveres every fact that it dare not overlook or falsify it, seem it never so minute; which feels that because it is a fact it cannot be minute, cannot be unimportant; . . . and which therefore, just because it stands in solemn awe of such paltry facts as the Scolopax feather in a snipe's pinion, or the jagged leaves which appear capriciously in certain honeysuckles, believes that there is likely to be some deep and wide secret underlying them which is worth years of thought to solve. But as for that other reverence which shuts its eyes and ears in pious awe . . . what is it but cowardice, very pitiable when unmasked; and what is its child but ignorance, as pitiable, which would be ludicrous were it not so injurious?"*

To return to the main subject. We wish for hypotheses which shall explain our observed or experimentally determined facts. If we are determined to do without hypotheses in our scientific method, let us see what is required of us. Given two circumstances, and one hundred other distinct circumstances which may possibly be connected with these, we are required to find, by mere inductive reasoning, the law regulating the coincidences existing between these circumstances. Now there are no less than 4950 pairs of circumstances, under the conditions just named, between which a coincidence may exist. We shall therefore be required to try these 4950 cases, in order to determine which of them represents the true grouping of the connected circumstances. Would it not be easier, after attentively looking at all the circumstances, to say, probably the coincidence lies here, and then try whether it does or not?

As an illustration of the vast number of combinations possible under certain circumstances the following is instructive:—In whist, four hands of thirteen cards each are simultaneously held: "The number of distinct possible deals is so great that twenty-eight figures are required to express them. If the whole population of the world—say a hundred thousand million persons—were to deal cards day and night for one hundred million years, they would not have exhausted in this time one one-hundred-thousandth part of the possible deals."† If we do not know anything of hypotheses, do not hazard at the least a guess, we shall very

* "Science." A Lecture delivered at the Royal Institution.

† Essay on Probability, by Lubbock and Drinkwater, quoted by Jevons, Principles of Science, vol. i., p. 217.

probably find ourselves—like the alchemists—spending our years in useless labour, searching in the labyrinth of detached facts without any guide, and, like them, we shall arrive at no true results.

The doctrine or theory of combinations enables us to determine the possible number of ways in which a given set of facts or circumstances may be grouped together. This theory is of the utmost importance in enabling us to form just conceptions of the nature of the task set before him who would investigate Nature. The importance of the doctrine of combinations is thus insisted on by James Bernouilli:*—"It is easy to perceive that the prodigious variety which appears both in the works of Nature and in the actions of man, and which constitutes the greatest part of the beauty of the Universe, is owing to the multitude of different ways in which its several parts are mixed with, or placed near, each other. But because the number of causes that concur in producing a given event, or effect, is oftentimes so immensely great, and the causes themselves are so different one from another, that it is extremely difficult to reckon up all the different ways in which they may be arranged or combined together, it often happens that men, even of the best understandings and greatest circumspection, are guilty of that fault in reasoning which the writers on logic call *the insufficient or imperfect enumeration of parts or cases;* insomuch that I will venture to assert that this is the chief and almost the only source of the vast number of erroneous opinions—and these, too, very often in matters of great importance—which we are apt to form on all the subjects we reflect upon, whether they relate to the knowledge of Nature, or the merits and motives of human actions. It must therefore be acknowledged that that art which affords a cure to this weakness or defect of our understandings, and teaches us to enumerate all the possible ways in which a given number of things may be mixed and combined together, and that we may be certain that we have not omitted any one arrangement of them that can lead to the object of our enquiry, deserves to be considered as most eminently useful and worthy of our highest esteem and attention, And this is the business of the *art or doctrine of combinations*. Nor is this art or doctrine to be considered merely a branch of the mathematical sciences, for it has a relation to almost every species of useful knowledge that the mind of man can be employed upon. It

* De Arte Conjectandi, translated by Baron Maseres (London, 1795, p. 353), quoted by Jevons in Principles of Science, vol. i., pp. 198—200.

proceeds, indeed, upon mathematical principles in calculating the number of the combinations of the things proposed; but by the conclusions that are obtained by it the sagacity of the natural philosopher, the exactness of the historian, the skill and the judgment of the physician, and the prudence and foresight of the politician may be assisted, because the business of all these important professions is but *to form reasonable conjectures* concerning the several objects which engage their attention, and all wise conjectures are the results of a just and careful examination of the several different effects that may possibly arise from the causes that are capable of producing them."

When we apply this theory to facts we are astonished at the results. Speaking of "the variety of logical relations which may exist between a certain number of terms," Prof. W. Stanley Jevons* says:—"Four terms give 16 combinations, and no less than 65,536 possible selections from these combinations; . . . for six terms the corresponding numbers are 64 and 18,446,744,073,709,551,616. Considering that it is the most common thing in the world to use an argument involving six objects or terms, it may excite some surprise that the complete investigation of the relations in which six such terms may stand to each other should involve an almost inconceivable number of cases. Yet those numbers of possible logical relations belong only to the second order of combinations."

If the facts of Nature be so numerous we can never hope to know them all. Perfect knowledge is for us impossible: how, then, are we to make the most of that partial knowledge which we can alone attain to? How can we measure the extent of our knowledge of any subject? By means of the theory of probability.

Although perfect knowledge is impossible, yet we cannot be content with the accumulation of mere isolated facts. We attempt to group facts together, to form theories, and to apply these to the explanation of newly discovered facts. The theory of probability must guide the mind in guaging its knowledge of any group of facts. And the theory of probability is, as Laplace has said, "good sense reduced to calculation."

Suppose it be required to determine the atomic weight of an element, we devise various methods of measurement, we repeat the measurements again and again, but there are nevertheless errors inherent in each method, errors in the

* Principles of Science, vol. i., p. 223.

instruments employed, and errors in our readings of these instruments, &c. The result can never be more than approximately correct, and the results obtained by the different methods will not be exactly the same. We do not therefore *know* the true atomic weight of the element; but the theory of probability enables us to assign to each result its comparative trustworthiness, and so to deduce the numerical probability of the average result being absolutely correct.

The application of the theory becomes often very difficult. Our knowledge is at the best so limited that it is difficult to assign to two propositions their relative probabilities. When we deal with simple numbers, as those obtained in the illustration given, we can apply the theory with comparative ease; but when we come to more complicated questions in physical science we find it almost impossible to obtain sufficient, and sufficiently reliable, data to enable us to estimate probabilities. But, as Prof. Jevons has pointed out, "Nothing is more requisite than to distinguish carefully between the truth of a theory and the truthful application of the theory to actual circumstances. As a general rule, events in Nature or Art will present a complexity of relations exceeding our powers of treatment. The infinitely intricate action of the mind often intervenes, and renders complete analysis hopeless. If, for instance, the probability that a marksman shall hit the target in a single shot be 1 in 10, we might seem to have no difficulty in calculating the probability of any succession of hits: thus the probability of three successive hits would be one in a thousand. But, in reality, the confidence and experience derived from the first successful shot would render a second success more probable. The events are not really independent, and there would generally be a far greater preponderance of runs of apparent luck than a simple calculation of probabilities could account for. In many persons, however, a remarkable series of successes will produce a degree of excitement rendering continued success almost impossible."

We must be content with partial knowledge.

In ascending from facts to generalisations, which generalisations are more or less probably true, we must make use of hypotheses; we must accumulate facts, make an hypothesis to explain them, and test the hypothesis by appeal to facts. The investigator of Science must begin with facts; he must end with facts; but between the two he must interpolate hypothesis. He looks at a number of facts; gradually he sees, or thinks he sees, a light dawning on him—a central idea, round which all the facts group themselves in a

luminous whole. But he does not stop here; he again appeals to facts. He says "If my hypothesis be correct, this or that fact must follow." Then he tries experiment. Is the predicted fact really a fact? By the test of experiment he is content to abide; he knows that Nature—however hard sometimes it is to make her answer at all—never answers except truly.

Hypotheses must be used in Science, but hypotheses may be abused. What, then, are the marks of a good hypothesis?

An hypothesis must be workable; it must not go against any well-established scientific generalisation, and it must be ready to submit to have its predictions proved by strictly experimental methods.*

A good scientific hypothesis must be workable; that is, it must allow us to make determinate predictions—predictions which can be proved or disproved by experiment. A vague generalisation, which does not allow of definite deductive reasoning, can have no place as a scientific hypothesis.

A good scientific hypothesis must not be opposed to any well-established generalisation of Science. This statement may probably be called in question by many. It is no uncommon thing to find people talking of the way in which Science sweeps aside all preconceived ideas, all Old World notions, all long-cherished delusions. And this is very true; only these people, I am afraid, forget the other side of the case; they forget that did Science present us with nothing but change succeeding change, doctrines swept away and replaced by others to be themselves removed as Science advances, Science would have no claim on our acceptance. It is because Science is at once prolific of changes, and conservative in the extreme, that she has accomplished her work in the world.

We know so little of Nature that we must be ready at any moment to give up that which we had supposed we did know; and yet we have such trust in the stability of Nature that we must cling to those theories which have been gained by slow accumulation of facts, until there is absolute experimental proof of their falsity.

Such a theory as that of the Conservation of Energy is the general expression of a vast number of facts: it explains these facts; it is a well-established generalisation of Science. If we are seeking to explain a number of newly-discovered facts, it is evidently our duty to frame an hypothesis which shall not be itself out of keeping with this theory of the

* Principles of Science, vol. ii., p. 139.

conservation of energy. For if we do not do so we are very probably assuming the incorrectness of that great body of facts upon which the theory rests. It would in most cases be almost better to distrust our personally-observed facts than to distrust so well-founded a generalisation. This is one view to take of the question. But, on the other hand, the theory of the conservation of energy is a theory only: it is probably true; we do not, and cannot, know whether it is or is not certainly true. If the observed facts, after the most careful observations, still remain unmoved, and if they are apparently opposed to the generally-accepted theory, the better method will doubtless be complete reservation of judgment until further experimental data is forthcoming. If the observed facts are, however, absolutely opposed to the theory, and if these facts cannot be gainsaid, then the theory must go; it has done its work, and must be supplanted by a wider generalisation. The scientific investigator must therefore cling to theory, and yet be ready to abandon theory at the call of fact.

It is, it seems to me, of the utmost importance to insist on this view of the work of the student of Nature; to declare that he trusts Nature altogether, but he distrusts his own powers of comprehending the workings of Nature; that he feels that all things are changing, but he nevertheless clings to what he can grasp of the changeless. The frame of mind of the man of Science is, then, at once opposed to those who would have us believe that "victorious analysis" has now at last reduced all things under her feet, and to those who would have us accept the teaching of authority in place of the teaching of facts. Both alike assume a vast amount of knowledge which neither is possessed of. But the last characteristic of a good scientific hypothesis is its readiness to submit to have its predictions proved by strictly experimental methods. Every newly-discovered fact which is capable of explanation in terms of an accepted hypothesis adds something to the *probable* truth of that hypothesis. Every newly-discovered fact which cannot be explained in terms of the hypothesis takes away something from the probable truth of the hypothesis. We may observe facts which are apparently opposed to the hypothesis which we have provisionally accepted, and yet we may not be justified in condemning the hypothesis, because these facts may either be but partially examined by us, or the hypothesis may not have been fully grasped in all its bearings. But if the hypothesis is to hold its ground there must be no experimentally demonstrated fact, the existence

of which would be impossible were the hypothesis correct. To take an instance:—The upholders of the Phlogistic theory affirmed that when a metal is burned it parts with phlogiston; that the product of combustion is metal *minus* phlogiston; and that the re-transformation of the product of combustion into metal is brought about by the absorption of phlogiston. The upholders of what might be called the Oxygen theory affirmed that when a metal is burned it combines with oxygen; that the product of combustion is metal *plus* oxygen; and that the re-transformation of the product of combustion into metal is brought about by the removal of oxygen. Each hypothesis had facts in its favour; each explained many facts. But the fact discovered by Davy, in 1807, that the metals potassium and sodium are actually produced by the removal of oxygen from those substances which are themselves formed when these metals are burned, could *not* be explained in terms of the phlogistic theory. Either the fact or the theory must give way. The fact was established beyond a doubt; therefore the theory—in its then accepted form at any rate—had to succumb.

A good scientific hypothesis must, then, be in keeping with facts; but it does not follow that it must be simple, or that it must make no claims upon our belief. The hypothesis which well explains the facts concerning light is, we might almost say, absurd in the demands which it makes upon our credulity. "We are asked by physical philosophers to give up all our ordinary prepossessions, and believe that the interstellar space which seemed so empty is not empty at all, but filled with *something* more solid and elastic than steel. As Dr. Young remarked, 'the luminiferous ether pervading all space, and penetrating almost all substances, is not only highly elastic, but absolutely solid.' Sir John Herschel has calculated the amount of force which may be supposed, according to the undulatory theory of light, to be exerted at each point in space, and finds it to be 1,148,000,000,000 times the elastic force of ordinary air at the earth's surface, so that the pressure of the ether upon a square inch of surface must be about 17 billions of pounds. Yet we live, and move without appreciable resistance through this medium, indefinitely harder and more elastic than adamant. All our ordinary notions must be laid aside in contemplating such an hypothesis; yet it is no more than the observed phenomena of light and heat force us to accept."*

* Principles of Science, vol. ii., p. 145.

Again, the hypothesis of Gravitation forces us to believe that a particle of matter here, on this earth, is at this moment acting upon each other particle of matter in the universe, and that apparently with an action to which time counts as nothing, and the mass of all the planets as a thin screen offering really *no* opposition.

When we come to examine the hypotheses of Science, we find that they have been developed to very varying degrees of perfectness. "Where, as in the case of the planetary motions and disturbances, the forces concerned are thoroughly known, the mathematical theory is absolutely true, and requires only analysis to work out its remotest details. It is thus in general far ahead of observation, and is competent to predict effects not yet even observed, as, for instance, lunar inequalities due to the action of Venus upon the Earth, &c., to which no amount of observation, unaided by theory, would ever have enabled us to assign the true cause. . . . Another class of mathematical theories, based to a certain extent upon experiment, is at present useful, and has even in certain cases pointed to new and important results which experiment has subsequently verified. Such are the dynamical theory of heat, the undulatory theory of light, &c. . . . . A third class is well represented by the mathematical theories of Heat (conduction), Electricity (statical), and Magnetism (permanent). Although we do not know *how* heat is propagated in bodies, nor *what* statical electricity or permanent magnetism are, the laws of their forces are as certainly known as that of gravitation, and can therefore, like it, be developed to their consequences, by the application of mathematical analysis."*

If it be impossible to group together the facts of Nature in every possible combination, and then to infer general laws; if it be necessary to make use of hypotheses, it may be asked—Is there no method applicable for forming these hypotheses? nothing to guide us in our guesses at Nature's laws? Of course it would be impossible to lay down *rules* for making hypotheses, just as it would be absurd to *teach* a man to be a genius; nevertheless, if we study the trains of thought by which the most eminent naturalists have been led to their great discoveries, we can arrive at some general idea of the methods which they have followed. These discoveries have evidently been guided by analogy. From one similarity, or from a few similarities noticed between different substances or between different sets of facts, they have

* Thomson and Tait, The Oxford Pamphlet, p. 110.

inferred the existence of more points of similarity; they have then framed hypotheses which have guided them in their subsequent experimental investigations. To take an illustration:—When an electric machine was worked a peculiar smell was noticed; when a stick of moist phosphorus was allowed to remain exposed to air, a similar smell was perceived; when a hot glass rod was dipped into a mixture of ether vapour and air, a similar phenomenon was perceptible. From these observed similarities Schönbein inferred that the cause of the peculiar smell was probably the same in each case, and following up this analogy by experimental investigation he discovered ozone—a substance which has played, and is doubtless destined to play, a most important part in general chemical theory.

Many instructive instances of the application of analogy are to be found in the science of chemistry; in fact that science is almost entirely founded on more or less general laws which have been deduced by analogical reasoning. The fact that certain elements form groups having many common properties, and more or less sharply differentiated from other groups, has long been known. The further fact that there is, in many instances, a regular gradation in the atomic weights of the members of such groups, seemed to point to a connection between atomic weight and general chemical behaviour of the elements. Many facts were in keeping with this assumption. The connection between chemical properties and change in atomic weight has of late years been much attended to; and it has been shown by Mendelejeff and others that, if the elements be arranged in order of their atomic weights, beginning with that which has the least atomic weight, the general properties—not only of the elements, but also of their compounds—may be regarded as functions of the atomic weights; that, moreover, these functions are periodic,—that is, that groups of elements may be formed in order of increasing atomic weights, and that the general relations existing between, say, the third member of group two and the other members of the same group correspond with those relations which exist between the third member of group four and the remaining members of this group. Following up the analogy, Mendelejeff has propounded an hypothesis which goes under the somewhat ambitious title of the *periodic law*, and from this hypothesis he has made certain predictions. Among other predictions he has foretold the existence of elementary bodies other than those we are acquainted with: he has even ventured to assign certain properties to some of these

hypothetical elements. Nor have his predictions been altogether unfulfilled. The most recent addition to the chemical elements is the metal gallium: in very many of its properties—in fact in its general chemical behaviour, so far as this has been experimentally examined—gallium corresponds very closely with one of Mendelejeff's hypothetical elements. We have here an example of an hypothesis founded on analogical reasoning.

But analogy may mislead; it has often misled men in framing hypotheses. As telescopes were made of greater and greater power, astronomers found that the nebulæ were resolved into clusters of stars. One by one these apparently gaseous masses were proved to be really aggregations of solid matter. Analogy suggested that all nebulæ would be resolved when sufficiently powerful instruments could be brought to bear upon them. But meanwhile a new method of research was discovered; and by the use of spectrum analysis Huggins has proved that certain nebulæ really consist of gaseous matter, and has therefore shown that the analogy in the structure of these bodies was not so complete as was supposed.

Analogy must evidently be used with caution. And here again we perceive the need of genius in Science. The ordinary man may amass facts, may even trace out a few analogies between groups of facts, but it is only the man of genius who will discover *the* analogy which will guide to great generalisations. Very probably even the genius will follow many false scents; but if he be a true student of Nature, besides being possessed of the divine gift of imagination, he will test his hypotheses framed on analogical reasonings by appeal to facts, and he will discover the true analogy and frame the correct hypothesis at last.

Of the vast masses of facts which are presented to the enquirer in each branch of Science there will be some of more value—considered as guides in deducing general laws—than others. Not unfrequently it happens that it is the fact which somehow refuses to fit in with the generally accepted hypothesis which becomes the means of guiding the investigator to a new and wider hypothesis. "When, in an experiment, all known causes being allowed for, there remain certain unexplained effects (excessively slight it may be), these must be carefully investigated, and every conceivable variation of arrangement of apparatus, &c., tried, until, if possible, we manage so to exaggerate the residual phenomenon as to be able to detect its cause. It is here, perhaps, that in the present state of Science we

may most reasonably look for extensions of our knowledge: at all events we are warranted by the recent history of natural philosophy in so doing."*

As an illustration of the use made by genius of "residual phenomena" I might cite the discovery of the planet Neptune by Adams and Le Verrier. Slight anomalies were observed in the motions of Uranus: these were studied; the hypothesis was framed that the peculiar movements were due to the presence of an unknown body; observations were carried out, and the new planet was discovered.

Almost every science presents us with residual phenomena awaiting explanation. To mention one in chemical science. Why are the densities of the vapours of phosphorus and arsenic twice as great, and the densities of the vapours of mercury and cadmium one-half as great, as all analogical reasoning would lead us to imagine they should be? Here is an unexplained fact which will doubtless one day be prolific of consequences.

I have thus attempted to sketch the main points in that method which has been, and is, pursued by scientific men in their attempts to discover the truths of Nature: in conclusion I must say a few words regarding the limits of scientific method.

In science we start with facts, we then form hypotheses which we test by appeal to facts. But so great is the number of facts presented to us that we cannot observe or experimentally determine more than a small, almost an infinitely small, portion of them. Much less can we hope to form satisfactory hypotheses which shall explain them all. This is true in physical science. The mass of facts gathered together by the naturalist is already extremely large; but there can be no doubt that the number of the unknown vastly exceeds that of the known facts of Nature. And of the known facts how few have as yet been explained. The problem of the "mutual effects of three bodies, each acting on the other under the simple hypothesis of the law of gravity," can scarcely be said to be yet completely solved. And if this comparatively simple case has puzzled the ingenuity of the mathematicians what are we to say to the application of mathematical processes to the explanation of those motions and mutual actions which we have reason to believe are being performed and undergone by the constituent portions of every chemical atom? Each of these particles, Sir J. Herschel has remarked, is continually solving differ-

* Thomson and Tait, The Oxford Pamphlet, p. 108.

ential equations, which, if written out in full, might perhaps belt the earth.

In physical science our ignorance is practically infinite as compared with our knowledge; and when we come to mental and moral phenomena we are almost without any data on which to base strictly scientific reasoning. Each human being presents the phenomenon of a mass of conflicting hopes, fears, desires, passions, and inclinations which science can never hope to classify. How shall we measure these mental phenomena? How shall we weigh accurately the emotions even of the least emotional of human beings? What units shall we employ? How shall we calculate the effects of each human life upon the general life of the community? We cannot hope ever to reduce these things within the grasp of rigid quantitative analysis. As Prof. Jevons has truly remarked:—"As astronomers have not yet fully solved the problem of three gravitating bodies when shall we have a solution of the problem of three moral bodies?"* And shall "victorious analysis" ever dream of attempting to bring under her formulæ the facts concerning man's relation to the physical world around him? If each set of phenomena, physical and mental, considered apart from the other, far surpasses our powers of investigation, how can science ever hope to approach the problem of the mutual relations of the two? "The air itself is one vast library, on whose pages are for ever written all that man has ever said or even whispered. These, in their mutable but unerring characters, mixed with the earliest as well as the latest sighs of mortality, stand for ever recorded—vows unredeemed, promises unfulfilled, perpetuating in the united movements of each particle the testimony of man's changeful will."† We cannot solve the mystery of the physical world, nor the mystery of the mental world, nor the mystery of the connection between the two.

But we do attempt nevertheless to lessen the sphere of our ignorance and to change the unknown into the known. We endeavour to explain facts by grouping them together under a generalisation. The wider generalisations of science are generally called laws. Having made a bold generalisation, having appealed to facts and found that our generalisation stands the test in any instance, we are very liable to conclude that this generalisation *must* hold good in all cases, and to give to the expression a *coercive* value. Indeed, the name *law* almost

* Principles of Science, vol. ii., p. 458.

† CHARLES BABBAGE, Ninth Bridgewater Treatise, p. 113.

implies coercive power. But are we justified in doing this? To say that the law must hold good in all cases implies infinite knowledge: we may have proved the law to apply in every instance which we have examined, but there is the chance that in the next instance it will fail. Prof. Jevons shows that "no finite number of instances can warrant us in expecting with certainty that the next instance will be of like nature." Every fresh instance of like nature to the preceding increases the probability that the law will hold good in all instances, but after all it is only a probability that we have gained. "The laws of Nature, as I venture to regard them, are simply general propositions concerning the correlation of properties, which have been observed to hold true of bodies hitherto observed. On the assumption that our experience is of adequate extent, and that no arbitrary interference takes place, we are then able to assign the probability, always less than certainty, that the next object of the same apparent nature will conform to the same law."*

We speak of matter obeying the law of gravity. In this proposition we imply the existence of two things—matter and force; matter, a *something*, acted on by *another something*, force. Of these two things we cannot give very good definitions. Matter is "that which can be acted upon by, or can exert force:" and force is "any cause which tends to alter a body's natural state of rest, or of uniform motion in a straight line."† But the force of gravity acting on particles of matter does not necessarily cause the actual approach of one body towards another; the action of this force upon a given particle of matter is conditioned by the number, mass, distance, and relative position of all the other particles of matter within the bounds of space at the instant in question. We must not forget that the action of the laws of Nature upon the matter of the universe is dependent upon the *collocations* (as Dr. Chalmers expressed it) of that matter at any moment of time. Given the same laws and the same mass of matter, but let the initial collocations of that matter vary, then the results would be altogether different for each collocation. No single law of Nature can be supposed to act independently of other laws. Every law is conditioned in its action by other laws. Or, perhaps, we should say that, in our ignorance, we are obliged to speak of special laws acting and reacting upon one another, when to infinite knowledge all would appear as under the control of but one

* Principles of Science, vol. ii., p. 431.

† Thomson and Tait, The Oxford Pamphlet, pp. 53, 54.

law. But by us, at any rate, various laws must be recognised; and these are mutually related. Now if we cannot hope to know all the facts of the Universe still less can we hope to comprehend all the laws thereof, and much less can we dream of arriving at a knowledge of the mutual actions of those laws upon one another, and the modifications in the action of one law upon material objects introduced by the interference of another law, or of other laws. And even our knowledge of individual laws is but approximative: the more carefully Nature is examined the more reason have we for disbelieving in the simplicity of her actions. At first everything appears chaotic; then facts group themselves together, generalisations are made, laws are framed. But after a time, as investigation proceeds, and as more exact methods are introduced, the law is found to be not quite in keeping with facts; the formula was only approximately true. There are slight exceptions, so slight that the older and ruder methods of research failed to detect them: the law is not rigorously exact. In the hands of the trained and able naturalist these small exceptions often prove stepping-stones to higher generalisations, which embrace in their enunciation the less widely applicable generalisation. But if every improvement in our methods of research serves to point out exceptions to what were formerly accepted as general laws, are we entitled to assume that we have *now* reached the true generalisation? Would it not be more becoming the spirit of true science to acknowledge our ignorance, to remember that while we have made one step nearer the goal that goal is itself still at an infinite distance from us?

I might illustrate this subject by reference to the researches of Caigniard de la Tour and Andrews upon the physical properties of gases, wherein it is shown that the laws in which Boyle, Marriott, and former experimenters enunciated the results of apparently complete investigations into the same subject were really only approximations to a solution of the problem. More recently Mendelejeff has shown, by very refined and laborious experiments, that Boyle's law is not strictly true, and he has paved the way for a higher generalisation. But space forbids me to enter into these details.

We generally regard a well established physical law as acting continuously throughout all past time. Of course this is merely an assumption, yet it is an assumption which is apparently necessary in most cases if we are to attempt a scientific solution of the problems of the Universe. But there are good reasons for believing that certain very well

established generalisations of science may not have held good during all past time. Sir Wm. Thomson has shown how to deduce (from Fourier's "Theorem of Heat") in certain cases the thermal state of a body in past time from its known condition at present, and one of the results of his investigation is the indication of "A certain date in past time such that the present state of things cannot be deduced from any distribution of temperature occurring previously to that date, and becoming diffused by ordinary conduction. Some other event beyond ordinary conduction must have occurred since that date in order to produce the present state of things. This is only one of the cases in which a consideration of the dissipation of energy leads to the determination of a superior limit to the antiquity of the observed order of things."*

It is possible to imagine a law which should exhibit a break, or breaks, of continuity. Babbage has shown that it is theoretically possible to devise a machine which shall work according to a fixed law for any finite period of time, and yet at a fixed moment exhibit a single breach of the law. The machine might, for instance, be constituted so as to continue counting the natural numbers for an immense period of time. "If every letter in the volume now before the reader's eyes," says Babbage, "were changed into a figure, and if all the figures contained in a thousand such volumes were arranged in order, the whole together would yet fall far short of the vast induction the observer would have had in favour of the truth of the law of natural numbers. . . . Yet shall the engine, true to the prediction of its inventor, after the lapse of myriads of ages, fulfil its task, and give that one, the first and only exception, to the time-sanctioned law. What would have been the chances against the appearance of the excepted case immediately prior to its occurrence?"†

In the application of scientific generalisations we assume that the future will be as the present; we overlook, necessarily, the chance of sudden interferences with the present order of things. Yet we have no ground for denying the possibility of such interferences. There are facts which make the existence of numerous dark bodies in space very probable. How do we know that by the collision of one of these unseen bodies with this planet the present order of things may not be suddenly terminated? Have we investi-

* Clerk Maxwell, Theory of Heat, p. 244, 245.

† Ninth Bridgewater Treatise, p. 140, quoted by Jevons, Principles of Science, vol. ii., p. 437.

gated all the hidden springs of energy within the earth itself? Is there no chance of a sudden outbreak of some kind which will destroy this world and all its inhabitants in the twinkling of an eye? These suppositions are not unscientific: it is unscientific to assume complete knowledge when we really know almost nothing.

I think I have said enough to show that the scientific method is necessarily limited; that it leads us to recognise our own ignorance and the vastness of the problems presented for solution.

In an early part of this paper I said that by the aid of science we rise from the changing to the changeless; but if what I have said concerning the limitations of scientific laws, and concerning the unknown possibilities of Nature be true, it would appear as if the firm standing-ground we had seemed to gain were vanishing from under our feet. In a sense it is so; in another, and higher sense, the ground remains sure and firm. Science, when we know our littleness and the greatness of Nature, exhibits to us the reign of law, but bids us beware of placing our partial interpretations upon her laws; she commands us to proceed in the investigation of facts, but to be very careful how we interpret these facts. We have learned enough already to lead us to believe that although we can never fathom the mysteries of the Universe, the Universe is nevertheless obedient to order. If that little portion of the Universe which Science has conquered to herself be so wonderful in its organisation and in its working what must the whole Universe be?

IV. CORNISH CHINA CLAY.*

By James Quick.

THE art of pottery being one of those branches of industry which supply the immediate wants of the human race, originated in the earliest period of mankind's existence, and at the present day no other of the useful arts has attained a higher degree of excellence in its various details or has such a universal demand for its numerous products. In discovering suitable materials for the pottery manufacture, as well as in perfecting the different processes of the industry, an extraordinary amount of talent in every age and in almost every nation has been expended, and for the antiquary and the historian the subject as a whole presents an inexhaustible mine of research. The story of Bernard Palissy in France, wasting his time, his energy, and his money in his efforts to solve the mystery of the white enamel, and that of Baron Böttcher—the chemist in Saxony whose unremitting perseverance through a long period of years was at last crowned with success through the merest accident—are vested with thrilling interest. The porcelain or white earthenware products of China and Japan, where the manufacture was probably started many years previous to the Christian era, had on account of their pure whiteness, their semi-transparent texture, and superiority of glaze, long been the envy of European artists, when Böttcher one day inspecting a white substance found in Saxony and newly introduced as hair-powder, was struck with its adaptability to the requirements of pottery, and afterwards identified it as being the much-coveted kaolin, or the same material as that used by the Chinese. Subsequently, kaolin in various degrees of purity was found in several other parts of Europe, and it is now known to consist of the felspar of white granite in a state of decomposition, and after passing through the raising process somewhat resembles ordinary whiting, but has a much greater density. The kaolin, or as it is more commonly called in England, "china clay," and sometimes porcelain clay, used at the Staffordshire

* In presenting this article for publication the writer wishes to express his sincere thanks to T. H. Stocker, Esq., of St. Austell; Thos. Kinsman, Esq., of St. Austell and Vounder; the Rev. C. M. E. Collins, of Trewardale, Bodmin; and those other gentlemen in Cornwall who have kindly afforded him facilities for obtaining information.

Potteries, and which is the chief constituent of all the porcelain or finer sorts of earthenware, is obtained wholly from the county of Cornwall and the adjoining district of Dartmoor. It is the purest or most highly esteemed description of the clay known, and may be called the mainstay of our English manufacture, without which we could not possibly hold our pre-eminence over other nations in the excellence of our products, and in a commercial point of view the china clay trade of Cornwall has a much greater importance than has hitherto generally been supposed.

The application of Cornish china clay to the purposes of pottery is due to the enterprising spirit of William Cookworthy, of Plymouth, whose persevering efforts in the cause of the ceramic art are only surpassed by those of Josiah Wedgwood, the "founder," or rather great improver, of the Staffordshire potteries. Mr. Cookworthy opened a pottery at Plymouth in 1733,* and probably used at first only the inferior clays of Dorset and Devon. Owing, however, to the meagre information that is now extant about Cookworthy's labours, the date when he discovered the china clay, or first applied it to any practical use, cannot be accurately ascertained, but from his travels through Cornwall as a chemist, and his peculiar aptitude for geological research, its existence is likely to have been known to him long before he thought of turning it to account. Possibly, too, he may have used the clay for some time privately, reserving his knowledge from the public until some favourable opportunity occurred of divulging it, in accordance with that jealous spirit of secrecy so characteristic of discoverers in the eighteenth century. It appears to have been about the year 1755 that he found a stone at St. Stephen's, Cornwall, which he proved to be identical with the Chinese *petuntz*, or as it is now called china stone,† used for forming the glaze on porcelain, and which is the felspar of granite in a less advanced stage of disintegration than when considered as "clay." From a short account of the life of Cookworthy, published by his grandson,‡ it appears that "he first found china clay and stone at Tregoning Hill, then in the parish of St. Stephen's, and afterwards in the domain of Boconnoc, the family seat of Thomas Pitt, nephew of the Earl of Chatham, and afterwards Lord Camelford." In 1768 Cookworthy, in company

* See a small pamphlet entitled Relics of William Cookworthy, by John Prideaux. London: Whitaker and Co., 1853.

† This was the first time that china stone was found in Europe.

‡ Memorials of Wm. Cookworthy by his Grandson, with an Appendix. London: 1854. Also another Appendix published in 1872.

with Lord Camelford and others, secured by patent* the exclusive use of the Cornish china clay and stone, and with these materials carried on the porcelain manufacture for five or six years at Plymouth, that town, of course, being able thereby to boast that it is the place in England where porcelain was first made. The adventure, however, ultimately proved a commercial failure, and in 1774 Cookworthy sold his patent to Richard Champion, a merchant of Bristol, and his works were transferred to that city. Soon afterwards Champion, in conjunction with other eminent potters, removed the undertaking to Tunstall in the Potteries; and thus raised to the zenith of its completeness our present wonderful and extensive British earthenware industry.† Cookworthy is said to have made several improvements in the details of the porcelain manufacture, and old Plymouth china is now held in high esteem by connoisseurs.

Of late years, also, the Cornish china clay has been applied to a variety of purposes besides that of making porcelain, and doubtless ere long many more channels of usefulness may open up for it, and Cookworthy's discovery even yet attain a still greater importance among our English commercial products. Indeed it is calculated that only about one-third of the quantity now annually raised is employed in the home manufacture of porcelain, and more than another third of the total quantity is exported to foreign countries. The clay, on account of its bleaching properties, is used for "sizing" cotton goods in the Lancashire and Cheshire districts, and also very largely among paper-makers. It is also used in France in the preparation of ultramarine, and in Germany in connection with the manufacture of gilt mouldings. It has also been alleged that on the Continent the kaolin is widely used for adulterating flour; although, if this be true, for the comfort of consumers it is well to know that the clay is perfectly harmless when taken in small quantities.

The district of Cornwall in which the largest deposits of china clay have been found is in the neighbourhood of St. Austell, in the parishes of St. Austell, St. Mewan, St. Stephens, St. Dennis, St. Enoder, Roche, and St. Blazey, and there are deposits on a smaller scale in the eastern part

* It may be remarked that Wedgwood violently opposed this patent, as he himself, notwithstanding his many improvements in pottery, would never entertain the idea of one for his own discoveries.

† Pottery had been made in Staffordshire many years previous to this, and Wedgwood had commenced his improvements about 1760, but only used inferior clays, until Champion introduced the Cornish kaolin to the Potteries. Coarse butter-pots were made at Burslem so early as 1650.

of the county, at Blisland and St. Breward, near Bodmin, and in the west near Helston. Lee Moor, a part of Dartmoor near Plymouth, also yields considerable quantities of the clay; but as the largest works there are held by merchants who are large raisers in the St. Austell district, it can hardly be said that the Lee Moor enterprise enters into any competition with the Cornish trade.

The natural state in which the clay is found cannot be better described than by the following extract from an able paper on "The China Clay and China Stone of Devon and Cornwall," read before the Society of Arts in May, 1876,* by J. H. Collins, F.G.S., a gentleman intimately connected with the subject:—

"In each of the granite masses which rise like islands in the sea of clay slate forming the western extremity of England, some portions have their felspar so decomposed as to be converted into kaolin, or china clay. Other portions are less decomposed, and of somewhat different composition, and these supply the china stone. These decomposed portions are always associated with veins of black tourmaline and other minerals containing fluorine. The mode of alteration of the granite rocks in these neighbourhoods, in my opinion, has certainly been effected by fluorine and other substances coming up from below, and not by carbonic acid and water acting from above. As a rule, the decomposition is more general near the junctions of the granite with the surrounding rocks than elsewhere, but to this rule there are some notable exceptions.

"The natural clay rock is almost always covered with a thick layer of stones, sand, or impure and discoloured clay, known as 'overburden.' This capping often much resembles glacial drift, but it never contains any scratched or glaciated stones or travelled blocks. This capping varies from 3 feet to 40 feet in thickness, and it must, of course, be removed before the clay can be wrought.

"The decomposed granite is found at all elevations except the very highest points of the districts, which are always composed of hard rocks; but yet their situation is usually indicated to the practised eye by a peculiar depression of the surface. These depressions are not observed in the case of china stone. The natural clay rock, being a decomposed granite, consists of kaolin, irregular crystals of quartz, and flakes of mica, with sometimes a little schorl and undecomposed felspar."

* A short and eminently practical work, duly reported in the Society of Arts Journal for May 5, 1876.

No generally accepted conclusion has yet been arrived at as to the direct causes of formation of the kaolin, or china clay. Mr. Collins is of opinion that it is formed through the action of fluorine, because fissures which invariably contain minerals (tourmaline and mica) of which fluorine is an essential ingredient are always present in the clay. Tin, too, is frequently found in connection with kaolin, which some geologists believe is brought there by the agency of fluorine. To this theory of formation we are strongly disposed to incline. Mr. Collins has himself acted upon granite with dilute hydrofluoric acid, and, without otherwise altering its appearance changed it into kaolin. It may be mentioned that most of the kaolin found in Europe consists, with slight variations, of $Al_2O_3,2SiO_2+2H_2O$.*

The other principal materials used in the preparation of porcelain are bones and flint, the latter being employed for hardening the ware.† Inferior descriptions of clay used in the Potteries are obtained from the counties of Devon and Dorset, especially from the neighbourhod of Teignmouth, where the much-used ball clay is found. At present there are 117 china clay works in Cornwall leased or rented from the freeholders of the land, and it has been roughly calculated that about 1600 working hands altogether are employed. In Devonshire there are eight different clay works, all in the vicinity of Lee Moor.

The method of raising the china clay in Cornwall is interesting. With the exception of a few modern improvements it is very similar to that pursued for many hundreds of years past by the Chinese clay raisers. And indeed many of the plans in clay raising and pottery manufacture invented and adopted by Europeans are merely unconscious repetitions of those practised long before by the workers in China and Japan. The clay is worked in open cuttings, which at some of the principal works are of very considerable extent and

* No very great amount, indeed, of scientific inquiry has yet been brought to bear upon the subject of the kaolin formation; nor does any chemical work to which we have been able to gain access bestow much attention on the point. WATTS'S Dictionary of Chemistry, vol. i., says "It (kaolin) may be supposed to be formed from orthoclase, or $K_2OAl_4O_36SiO_2$, by the abstraction of the whole of potash and $\frac{3}{4}$ silica, and addition of 2 at. water," but offers no suggestion as to how this may by nature be brought about. Watts also says "the averagecomposition (of kaolin as naturally found) is $Al_4O_3,2SiO_2+2Aq$," but Ure's, Tomlinson's, and other scientific cyclopædias agree with the formula $Al_2O_32SiO_2+2Aq$, which latter is the most correct, so far as the *major* quantity of kaolin found is concerned.

† Pegmatite, a substance found rarely, but which deserves more attention from geologists than has hitherto been bestowed upon it, is said to contain in its natural state all the necessary ingredients for porcelain.—See TOMLINSON'S Cyclopædia of Useful Arts.

depth, the latter varying from 30 to 120 feet. Over the sloping sides of these workings streams of water are directed which in their course carry with them the deposit of clay mixed with sand and mica, the proportions varying at the several works, but averaging about 8 tons of sand and mica per ton of china clay. During the progress of the streams down the sides—or, as they are locally called, "stopes"—of the cuttings, shovels are used for causing the separation of the clay and sand, and the several streams meet at bottom, carrying with them the liquid mixture into a reservoir, where the sand and much of the mica becomes deposited. At some works the liquid is then pumped, and at others it flows from the reservoir by natural gravitation into a series of long channels called "micas." These are from 2 to 6 feet wide and from 4 to 9 inches deep, varying with requirements and size of streams to be passed, and in length from 100 to 500 feet. The channels have a slight fall, and have weirs or dams placed about 20 feet apart, which, of course, check the stream at intervals and cause the heavier portions of the mixture to fall to the bottom, and these latter are afterwards run off and re-washed, or sent away as refuse.* Passing from the "micas," the solution, being clay alone, flows into large "tanks" averaging about 40 feet long, 50 feet wide, and 6 feet deep, where it settles, the water running off as the deposit becomes denser and the tank fills. If the clay is to be dried in the open air the thickened clay is then run off from the tanks into "pans," which are excavations of from $1\frac{1}{2}$ to 2 feet deep, of various sizes, and sanded at bottom. When sufficiently solid the clay is cut into square blocks and ranged in rows on the ground, and, being exposed to the influence of sun and wind, soon becomes dry. Of late years, at some works, kilns for drying the clay have been introduced: they vary in length from 100 feet and upwards; the clay is conveyed to them from the tanks by various methods, and their drying capacity differs according to circumstances and requirements.

The arrangements for transporting the clay from one part of the county to another are almost as complete as can be

* A marked feature of the Cornish county, and also of parts of Devon, are the white and red streams of refuse water which respectively are discharged from the clay works, and the iron mines, and flow through the green fields and valleys till they reach the sea, where they often form a serious obstacle to the prosecution of the coast fisheries. The other materials (much of which is used for inferior purposes, such as tiles, chimney-pots, fire-bricks, &c.) dug out whilst excavating china clay are overburden (common earth, rubble, &c.), fine mica, coarse mica, discoloured clay.

desired. The Cornwall Minerals Railway, running from Par to Newquay, passes through the important clay district of St. Austell, and connects the English and Bristol Channels and the Cornwall Railway, stretching the whole length of the county, has many small branches for conveying mineral and other commodities. It is noticeable that the clay sent to the English Potteries is taken from Cornwall wholly by water, that for Staffordshire being shipped to Runcorn, and that for Worcestershire to Gloucester, from which places the material is carried by canal; and this custom of transporting the clay by shipping probably arose from the strong preference shown by Wedgwood for water-carriage for porcelain rather than carriage by land, and his remarkable efforts in furthering the cause of canal navigation in the neighbourhood of the Potteries. The quantities transported through Runcorn in 1876 were 50,222 tons of china clay and 10,633 tons china stone; those through Gloucester were 2456 tons china clay and stone. The following are the amounts* of clay and stone sent from Cornwall in 1876, the places mentioned being the only ports of shipment. Owing to various causes, to which we shall presently refer, the total quantity is said to be less by many hundreds of tons than the average exports for previous years. From Par 35,000 tons of clay and stone were shipped coastwise and 15,000 tons to foreign countries; from Fowey, 61,070 tons coastwise and 14,760 tons foreign; from Charlestown, 34,016 tons coastwise and foreign; from Pentewan (clay only), 4727 tons coastwise and 8786 tons foreign; from Falmouth, coastwise 15,043 tons and 3395 tons foreign, clay and stone; Newquay, total 7,200 tons coastwise and foreign; from Wadebridge, total 2727 tons; from Plymouth, Sutton Harbour, 2345 tons coastwise and none foreign; and from Plymouth Great Western Dock 28,093 tons: total, 232,160 tons of china clay and stone exported from the various works in 1876. The prices of the clay and stone vary according to the purity of the article and, of

* Kindly supplied by the respective harbour-masters; but owing to the different methods of keeping accounts at the various harbours it is impossible to distinguish in each case the amounts of china clay from china stone, or the quantities sent coastwise from those exported to foreign countries. The clay shipped from the Plymouth Great Western Dock comes wholly from the neighbourhood of Dartmoor; that from Sutton Harbour, Plymouth, comes partly from Dartmoor and partly from Cornwall. It must also be understood that the figures here given do not represent the quantities actually *raised* in 1876. Many tons probably of the quantities given were kept over from 1875. Some interesting particulars about Cornish china clay and stone may be obtained from the "Mineral Statistics," published by Prof. Hunt, of the Museum of Practical Geology, Jermyn Street, W.

course, the many circumstances which influence commercial markets. They at present (July, 1877) range from 15*s.* to 30*s.* f. o. b.

As in excavating and preparing the clay no great amount of skilled workmanship is required, only men of the common labourer class are as a rule employed. These are generally under the superintendence of foremen, or, as they are locally called, "captains," who are men that have raised themselves by ability and experience. The wages of the men are 2*s.* 6*d.* for a day's work of seven and a half hours, a rate of pay which—especially when considering the abundant time afforded for extra work, and the facilities in Cornwall for cheap living among the poorer classes—compares most favourably with the wages earned by labourers of other avocations. The men, too, are not checked for rainy days when they cannot work, and the occupation is a very healthy one. In some parts also a system of piecework is adopted, under which the men usually make from 3*s.* to 3*s.* 6*d.* per day. With these earnings the Cornish labourer—renting perhaps a small respectable house for about £3 or £4 per annum, and, if he be married, his wife and children probably being also employed in the clay works, or in connection with the neighbouring tin or copper mines—may be said to rank in the social scale with skilled artisans in many other parts of England.

Until within the last two or three years the relations between the men and their employers had been noticeably good, but in the fall of 1875 the masters, as the trade was very dull, decided upon reducing the rate of wage by 3*d.* per day. This was strongly and openly objected to by the men, and ultimately the rate was allowed to remain at 2*s.* 6*d.* A Trades' Union was then formed among the labourers, ostensibly for regulating the "rates of wages and the hours of work," although from its subsequent proceedings it would seem rather to have been for annoying the masters as much as possible in revenge for their attempt to shorten pay; and although by no means wishing to depreciate the value of Trades' Unions generally, yet we must certainly say that this short-lived union presents an example of the excess of folly to which well-intentioned but misguided men can be urged by the representations of factious agitators. Notwithstanding, too, that several months have now elapsed since the strike occurred and the animosities excited by it have mostly subsided, yet we think a brief and impartial statement of the facts of the matter here to be not alto-

gether inappropriate, even if it only serves to exhibit the sturdy independence and almost stubborn fixidity, when once a purpose is resolved upon, of the Cornish character, glossed over, as that character is, by a spirit of extreme enthusiasm for whatever is undertaken. For our friends on the western side of the Tamar, in spite of the extinction of their language and the gradual decay of Cornish superstitions and prejudices, have still some remarkable traits of disposition quite as distinctive as those of any native of the Land o' Cakes or the Emerald Isle. Doubtless had the masters, during the time of the strike in question, been other than Cornish ones, the men would have obtained compliance with their demands long before they carried their actions to such an excess of riot as was reached; and had the labourers been other than Cornishmen, affairs, under the determined measures of the masters, would have assumed a pacific appearance much sooner than they did. Emboldened by the "big talk" of those persons who preferred hearing the sound of their own voices to performing honest labour, the members of the Union, towards the close of 1876, arranged to strike work on a certain day, and it was also agreed that those non-unionists who determined to work on that day should be forcibly prevented from so doing. Notices to this effect were posted up in the various towns in the St. Austell district, and accordingly, on the day in question, the unionists refused to work, and some most disgraceful scenes occurred.* The non-union men had, in the words of a local paper, to "cut and run," many of them having to hide for hours from the vengeance of the unionists. The men on strike visited the various works where the non-unionists were employed, obliging the latter to discontinue their occupation. This was done for several days, and although few deeds of violence were actually perpetrated, threats were very freely used, and the non-union men were in a state of great alarm. On the ringleaders of the strike being apprehended, one of them was fined £5 and costs for

* The following extract from a letter which appeared in the "Plymouth Daily Mercury" during the time of the strike above described, may serve as an instance of the excess of violence to which the Cornish working classes can allow themselves to be driven. It refers to a strike among the tin and copper miners some years previous. During the strike in East Cornwall it was "considered a trivial matter to place a poor fellow across a pole, and to shoulder him through the village of Gunnislake until the life was nearly jolted out of his body, to the delight of the leaders of the strike. The magistrates of Callington, however, took a different view of the matter, and Bodmin Gaol had in consequence some extra occupants for a considerable period after the strike had died out."

pointing out a certain individual for some other man to obstruct, another was sentenced to five weeks' hard labour for breaking the collar-bone of a non-unionist, and the president of the union got ten weeks' hard labour for going about encouraging the crowd in their acts of intimidation. The masters stated their determination to employ whom they pleased, and they preferred to employ only non-union workers; and this they did. Many of the malcontents returned to work at the old rate of wages, but a large proportion emigrated to Australia and other places.

During the last three or four years the Cornish clay trade has experienced a state of almost unprecedented depression, and, owing to the many new sources of clay supply which have of late years sprung up in various parts of Europe and elsewhere, grave doubts are entertained by some as to the future prospects of the Cornish trade.* Extensive works have been opened in Allier, Finisterre, and other departments in the north of France, and these now supply many of the French potteries where formerly Cornish clay was used. The clay districts of the eastern parts of the United States have also been more largely worked of late, the demand for the Cornish material in that country being thereby greatly diminished. The kaolin deposits, too, in the island of Bornholm now supply largely many of the paper-mills in Sweden, Denmark, and North Germany, and lessen by many hundreds of tons the annual exports from the Cornish districts. These misgivings as to the state of trade, however, do not appear to be shared by the majority of Cornish clay merchants, and an impartial consideration of the facts of the case must, in our opinion, lead to the conclusion that there is—at least as yet—very little ground for apprehending any permanent depression. We think the present unsatisfactory condition of affairs is owing chiefly to the unusual but now long-standing dulness of trade generally, and especially of the pottery trade and other departments of commercial enterprise in which the clay is employed; and when these branches of industry shall have revived, as they already are beginning to do, the Cornish clay trade will doubtless gradually recover itself. Another special cause of the present depression of the Cornish trade was over-speculating, in the years 1872-3. In those years many new clay-works were opened up by persons or

* See letter in the "Plymouth Mercury" of January 17th, 1877.

companies who did not understand the nature and proper modes of working china-clay works, or who were ignorant as to what could and what could not be worked at a profit. The result with such people is, that after large expenditure they find themselves overstocked with china clay, raised at heavy cost, and for which they cannot find a sale at remunerative prices; and their clay is now being disposed of for what it will fetch, some even being sold by auction at a merely nominal value, either to wind up the concerns, if they be public companies, or to get back money for pressing emergencies to those who have not sufficient capital to withstand the fluctuations of the market. At present, too, the habit of "forcing the royalties" forms a serious obstacle to the progress of the clay trade. The royalty is a certain sum per ton payable to the landowners by the lessees on all clay raised. The lessees or renters of the land generally pay a fixed minimum rent, which is allowed to merge into the royalty until the royalty exceeds the minimum rent, when the former is paid no matter what amount it may reach. Some of the lessees who have very large works, and whose leases are soon to expire, have been forcing their outputs of clay, and selling at a very reduced rate, in order to gain favour with their landlords by raising the royalties to as large a sum as possible beyond the minimum rent so as to obtain new leases on favourable terms. This method of proceeding is a well-known fact only to the few concerned, but the wisdom of a course which it is plain must seriously damage the market is certainly very questionable. The decrease in the demand for the Cornish article, owing to the more extensive use of native clays in America and other parts, will probably be now soon counterbalanced by the increased development in those branches of industry in other places where the home clay is employed. And even if there should be some cause for alarm it ought only to stimulate the Cornish proprietors to discover fresh means of disposal for their material. The scheme suggested a few years since for establishing a local industry by the introduction of the manufacture of porcelain *in situ* or into Cornwall instead of

* The fixed royalty dues vary from 2s. 6d. to 4s. per ton. The system of royalties may perhaps be made clearer from the following illustration:—Suppose the minimum rent is £100, and the dues 4s., the tenant must raise 500 tons of clay (at 4s. per ton) to cover his rent. Whatever quantity under 500 tons is raised he *must* pay £100; but whatever quantity *over* 500 tons is raised he would pay 4s. per ton—thus 1000 tons, say at 4s., £200—hence the rent is said to merge in the dues or royalty.

sending the raw commodity to the Potteries deserves to be more attentively considered than it has been hitherto. This scheme was originally brought forward by the Rev. C. M. E. Collins, of Trewardale, Bodmin, in the year 1868, in an able lecture on the subject delivered by that gentleman before the Royal Polytechnic Society of Cornwall (and duly reported in the Society's Journal, October, 1868), in which the feasibility of such an undertaking was fully demonstrated. The crucial difficulty urged against the Rev. Mr. Collins's proposals is that of the want of coal, as all coal, of course, used in the county has to be carried there from other parts. But Mr. Collins has shown that the amounts now expended annually in sending the clay to the potteries, and again in carrying the manufactured articles from the potteries to Liverpool for export, and in keeping agents in the potteries by the Cornish firms, far exceeds in any average period the outlay that would be entailed in the single article of coal. Indeed, we think numerous advantages would, on the whole, result from the adoption of Mr. Collins's plans, not only to those immediately connected with the Cornish clay trade, but also to the public generally; for Cornwall seems eminently fitted to be the seat of a pottery manufacture, all the porcelain now made at the potteries not only having to leave the county in the shape of the raw material, but also mostly having to pass her shores when being exported to foreign countries, and she has harbours well suited for accommodating any vessels that might be engaged in the carrying trade, and a large amount of sea freight and risk could be saved. By the new enterprise fresh improvements, too, in machinery and other matters could be introduced, which either prejudice or an unwillingness to incur expense prevents many of the present owners of the potteries from adopting, and the final result would doubtless be that an even better description of pottery than that now made could be produced at a much lower price. Mr. Collins calculates that to start his scheme in full working order would require about £30,000.

Already inferior articles, such as fire-bricks, tiles, &c., are manufactured in Cornwall, and an earth is found in the county well adapted for making the *seggars* or ovens in which the porcelain is baked. Of late, also, articles of an ornamental as well as useful character have been manufactured from china clay and stone, both in their raw state (or without undergoing any heating process), and after being made into porcelain paste, and there seems to be every likelihood that the clay may be still more extensively employed for the

purposes of decorative art. Statues, mortuary tablets, ornamental chimney-pieces, dressing-tables, blocks for church-pillars, and many other articles, are now made from the clay or stone, and some Mosaic floor-work* in china clay, recently manufactured, is considered to rival in appearance Devonshire alabaster.

All things being taken into account, we are of opinion that if the Cornish clay merchants continue to manifest that sage firmness of conduct which has characterised them under recent difficulties, their trade will soon emerge from its present depressing crisis and regain its former flourishing condition, and we strongly urge clay proprieters and all other persons interested in the welfare of the manufacturing arts, and desirous of profitable investment, to bestow their gravest attention on the scheme we have just discussed, by the adoption of which the clay business as a whole would, beyond a doubt, reach an even higher degree of prosperity than it has ever before attained.

V. THE SIGNIFICANCE OF THE PHENOMENA OF ONTOGENESIS IN REFERENCE TO THE EVOLUTION HYPOTHESIS.

By J. Huddart.

To afford a due appreciation of the importance of the relation of individual to racial development it will be necessary to review briefly some of the leading opinions on the Theory of Evolution, and to observe their tendency.

While most of our biologists and physicists agree that no better explanation can be advanced for observed facts than that all existing and extinct species of animals and plants have been evolved from one or more primordial forms, they are by no means in accord as to the agent or agents that have effected this evolution.

The professors of what is commonly known as the Darwinian Theory, but what might be more properly termed

* Specimens of this description of work in china clay may now be seen in the South Kensington Museum.

the Doctrine of Descent, may be broadly divided into two classes—one of which comprises those who claim to be the exponents of the Darwinian view of Evolution, and who regard all structural changes as due to natural and sexual selection and other casual causes; and, in general, consider all organic processes, development, growth, and the modification of species to be the result merely of a very refined and complex action of the molecular forces.

Oscar Schmidt, who is one of this class, states his position very clearly in the "Doctrine of Descent and Darwinism." He remarks that the physical view in its perfection reduces every organic process to a problem of pure mechanics, and quoting A. Fick—"I am of opinion that the mechanical view of organic life is demonstrated only when all the motions in an organism are shown to be the effects of forces, which at other times also are inherent in its atoms. But, similarly, I should regard the vitalistic view as proved if in any case a particular motion actually observed to take place in an organism were shown to be mechanically impossible. . . . . I provisionally profess myself unequivocally in favour of the mechanical view." I was surprised to find in this work of Oscar Schmidt's a quotation from the "Origin of Species," in many points resembling the concluding remarks in a paper of mine contributed to the last January number of this Journal. Although I do not remember having read the passage in the "Origin of Species" prior to writing the paper I refer to, I think I must have been unconsciously influenced in some way by the memory of Darwin's eloquent words, which are as follow:—"There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms or into one, and that whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms, most beautiful and most wonderful, have been and are being evolved." "In this concession," Oscar Schmidt remarks, "Darwin has certainly been untrue to himself [*or to his followers' interpretation of his theory ?*], and it satisfies neither those who believe in a personal God nor the partisans of natural evolution. It is directly incompatible with the doctrine of descent [*according to the physical view*], or as Zöllner says:—'The hypothesis of an act of creation (for the beginning of life) would not be a logical, but a merely arbitrary, limitation of the causal series.'" Again, he remarks:—"To anyone who holds open the possibility that even now animate may be evolved from inanimate existence without the mediation

of progenitors, the first origin of life in this natural method is at once self-evident. But even if the proof were given, which never can be given, that in the present world spontaneous generation does not occur, the inference would be false that it never did." It is evident from these remarks that Oscar Schmidt is fully sensible of the important bearing of the question of spontaneous generation on the mechanical theory of life, a subject we shall discuss later in this paper.

The mechanical view of evolution is held by Professors Huxley and Tyndall, and by Mr. Herbert Spencer and others.

We have referred to another class of evolutionists. It is represented by those who, while admitting the sufficiency of natural selection and other extraneous causes to account for a wide range of facts, do not consider that they are adequate to explain *all* the phenomena connected with animate nature. Apart from this general agreement their views do not altogether coincide. The following short descriptions of some of the most important opinions will greatly facilitate the reader in forming an estimate of the value of the conclusions hereinafter arrived at, as it will be seen that these opinions (notably that of Mr. Wallace), derived from general considerations, coincide in a remarkable manner with the results deduced in this paper from independent sources.

As is well known, Mr. Wallace, independently of Mr. Darwin, elaborated the theory of natural selection, but he differs from Mr. Darwin in some important particulars. He does not consider natural selection adequate to account for the cerebral development of a savage, who, while possessing a brain little inferior in size to that of a philosopher, has mental qualities very slightly superior to those of some of the higher mammals. Thus the enlargement of the brain, which is equivalent to an increase of mental capability, is unnecessary to the savage and in advance of his requirements, and is therefore due to some cause other than natural selection. A reserve of mental capacity has been accumulated in the savage, which renders him capable of rapid civilisation and improvement. Natural selection fails also to account for the absence of hair on the human body, as its loss could in no wise have been beneficial [*Darwin, however, considers this to be due to sexual selection*]. The extreme development of the larynx in the female sex could never have been acquired by savages, as it is beyond their wants, and their known habits could not have effected it. From these and other considerations, such as the want of relation between a number of man's mental qualities and his material

progress, Mr. Wallace makes the following very remarkable deductions;—"That a Superior Intelligence has guided the development of man in a definite direction. . . . Even if my particular view be not the true one, the difficulties I have just put forward remain, and I think prove that *some general and more fundamental law underlies that of natural selection*. . . . It is probable that the true law lies too deep for us to discover it; but *there seems to me to be ample indication that such a law does exist, and is probably connected with the absolute origin of life and of organisation*." In a note to this paragraph he says that some of his critics have accused him of believing that "our brains are made by God and our lungs by natural selection." This would be a legitimate objection if true, for if there be a law underlying that of natural selection it is universal; we have but to search for its workings, to trace its operations, and we shall find them throughout organic nature; not only in the human cerebrum, but in every part of every living thing, both animal and vegetal. This is, I think, also Mr. Wallace's belief.

Mr. St. George Mivart believes in an internal tendency to perfection. In his "Genesis of Species" the following remarks occur:—"Now it is here contended that the relationships borne one to another by various component parts imply the existence of some innate internal condition, conveniently spoken of as a power or tendency, which is quite as mysterious as is any innate condition, power, or tendency resulting in the orderly evolution of successive specific manifestations. These relationships, as also this developmental power, will doubtless in a certain sense be somewhat further explained as science advances."

Even Mr. Darwin, with characteristic candour, writes:—"I now admit, after reading the essay by Nägeli on plants, and the remarks by various authors with respect to animals, more especially those recently made by Professor Broca, that in the earlier editions of my "Origin of Species" I probably attributed too much to the action of natural selection, and the survival of the fittest. I have altered the fifth edition of the 'Origin' so as to confine my remarks to *adaptive changes of structure*."

Professor Owen, while acquiescing in the doctrine of descent, does not accept the theory of natural selection *in toto*.

Thus there seems to exist a tolerably general sense of insufficiency in the theory of selection, and the tendency is, in a great measure, to return to views approaching those of

the great French naturalist who first systematised the evolution hypothesis, crudely and imperfectly it is true, but with what breadth and grasp of generalities! Lamarck, perhaps, attained nearer to the truth than some of those to whom the glory belongs of having in late years elaborated the great theory of Natural Selection, and of having revived interest in a subject, the importance of which cannot be over-estimated. He believed in a primary developing cause, of the nature of which he had no conception, and in a form of spontaneous generation, because he could not otherwise account for the beginning of life: to quote his own words—"That all the organisms of the world are the true productions of Nature, and that they have been very gradually evolved. That Nature, in her course, commences and ever re-commences, by forming the simplest organisms, and that she only directly forms these, that is, the first germs of organisation, which have been designated 'spontaneous generations.'" He also maintains that Nature has implanted in the first germs of life a faculty of growth. It would appear that he does not intend to identify the agent he calls Nature with mechanical causes. The following quotation from the Introduction to his "Philosophie Zoologique" seems, however, to point to a contrary conclusion:—"That it is, unquestionably, a very great and profound truth, that all operations, moral and physical, are due to the same source. . . . And, in short, that all the phenomena of the intelligence and the will have their origin in the primitive and accidental state of the organism." Although not very clear as to the cause he has no doubt as to the fact of a progressive development having taken place since the commencement of life in the world; "a fact," he says, "which deserves all the attention of those who study the nature of animals; a fact partially seen many centuries ago, never completely grasped, always exaggerated." He then proceeds to point out that the reality of the progression does not need to be established by an act of reasoning, but is simply an observed phenomenon. He strongly repudiates the opinion, which he complains had been attributed to him, that animal and vegetable life had a common origin, and he also declares his belief that living cannot have sprung from not-living matter. His conception of spontaneous generation must therefore have differed considerably from modern notions on the subject. He remarks that the primary cause has been disturbed here and there in its action by a casual, and consequently varying, secondary and modifying cause.

As yet, as far as I can discover, no evolutionist has pointed out the full significance of embryological phenomena in reference to the Evolution hypothesis.

The analogy between the development of the individual and the unfolding of the primordial germ has been regarded hitherto only as a corroboration of the doctrine of Descent.

All organisms commence their existence, *de novo*, from the lowest types. Instead of at once developing the parental characteristics they pass through metamorphoses similar to those which have, in past ages, succeeded each other in the progressive development of their families.

This very striking parallelism of phenomena suggests, I think, the true solution of the mystery of Evolution.

The fact of the orderly reproduction of ancestral forms in the phases of individual development is so general that there can be no question as to its being a fundamental law of Nature. In order to illustrate its universality, I have chosen a few extracts from Dr. Carpenter's "Human Physiology," and from the work of Oscar Schmidt already referred to.

The first condition of the primordial germ is common to all animals, and is permanent in the lowest forms: there is no indication at this stage to what form of organic structure it may ultimately attain. Development commences by duplicative subdivision; a mass of cell-like bodies is thus produced whose component parts are all alike. This phase also has its parallel among the simpler organisms (the *Hydra* and *Planaria*). In all vertebrate animals the appearance of the primitive trace indicates the primary division to which they belong: this trace represents the vertebral column, and is first laid out as an unsegmented cord and an unsegmented sheath for the spinal cord; this is the permanent state of the lower fishes. In the development of the Vertebrata the vitelline vessels correspond to the mesenteric veins of invertebrated animals, and the blastodermic vesicle is to be regarded as the temporary stomach of the embryo, remaining as the permanent stomach in the radiated tribes. The circulation of Mammalia is at first carried on exactly on the plan which is characteristic of the circulating apparatus of fishes. The aorta subdivides on either side of the neck into three or four arches, which are separated by fissures much resembling those forming the entrances to the gill-cavities of cartilaginous fishes; and these arches re-unite to form the descending aorta, which transmits branches to all parts of the body. In the higher Vertebrata, however, the plan of the circulation is afterwards entirely changed by the

formation of new cavities in the heart, and by the production of new vessels. The clefts between the branchial arches close up and disappear. In the amphibians the branchial arches really bear gills during the larval state. A temporary respiratory apparatus, the allantois, bearing a strong resemblance in its own character, and especially in its vascular connections, to the gills of the Mollusca, is developed at an early stage: it seldom attains any size in Mammalia, being early superseded by another provision for the aëration of the blood. Many of the malformations occasionally observed in man are for the most part due to arrest of development; the circulating apparatus is sometimes permanently fixed in conditions that are properly characteristic of cold-blooded animals: and it is interesting to remark, too, that the varieties which not unfrequently present themselves, in the arrangements of the principal trunks given off from the aorta, find their analogues in the arrangements that are normally characteristic of some one or other of the Mammalia. The venous system of the human body undergoes changes which are even more remarkable than those of the arterial trunks. In its earliest condition it has been ascertained by Rathke to present essentially the same type in the embryos of all vertebrated animals, the peculiarities of each group being acquired by a process of subsequent transformation. There is at first a pair of anterior venous trunks, receiving the blood from the head, and a pair of posterior trunks, formed by the confluence of the veins of the trunks, of the Wolffian bodies, &c.: the former are persistent as the jugular veins; the latter remain separate in most fishes, where they are designated the Cardinal Veins; but in man (as in warm-blooded Vertebrata generally) they are only represented by the venæ azygos, major and minor, which coalesce into a common trunk for a considerable part of their length. One of the anterior trunks and one of the posterior unite on either side to form a canal, which is known as the Ductus Cuvieri; and the ducts of the two sides coalesce to form a shorter main canal, which enters the auricle, at that time an undivided cavity. This common canal is absorbed into the auricle at an early period in all Vertebrata above fishes; and after the septum auriculorum is formed, the two Cuvierian ducts separately enter the right auricle. This arrangement is persistent in birds and the inferior mammals, in which we find two venæ lavæ superiores entering the right auricle separately; but in the higher Mammalia and in man the left duct is obliterated, and the right alone remains as the single vena lava superior,

a transverse communicating branch being formed, to bring to it the blood of the left side. The double vena lava sometimes presents itself as a monstrosity in the human subject; some vestiges of the original arrangement may be traced even in the normal condition of the venous system in the adult. The embryological development of the urinary organs in vertebrated animals is a subject of peculiar interest, owing to the correspondence which may be traced between the transitory forms they present in the higher classes and their permanent conditions in the lower. Vessels exactly analogous to the corpora malpighiana of the true kidney remain as the permanent urinary organs of fishes, but in the higher Vertebrata they give place to the true kidneys. At the end of the third month (in the human subject) the kidneys consist of seven or eight lobes; their excretory ducts still terminate in the canal which receives those of the Wolffian bodies and of the Fallopian tubes; and this opens, with the rectum, into a sort of cloaca, analogous to that which is permanent in the oviparous Vertebrata. In the embryo the distinction of sexes is altogether wanting at first, and the conformation of the external parts of the apparatus is originally the same in man and the higher Mammalia as it permanently is in the oviparous Vertebrata. A partial representation of the phase of development at the tenth or eleventh week in the human subject is found in the permanent condition of the Struthious birds and of the implacental Mammalia. In the small proportion which the cerebral hemispheres bear to the other parts, in the absence of convolutions, in the deficiency of commissures, and in the general simplicity of structure of the whole, there is a certain correspondence between the brain of the human embryo at about the sixth week and that of a fish. About the twelfth week we find the cerebral hemispheres much increased in size, and arching back over the thalami and corpora quadrigemina; still, however, they are destitute of convolutions, and are imperfectly connected by commissures, and there is a large cavity yet existing in the corpora quadrigemina which freely communicates with the third ventricle. In all these particulars there is a strong analogy between the conditions of the brain of the human embryo at this period and that of the bird. Up to the end of the third month the cerebral hemispheres present only rudiments of anterior lobes, and they do not pass beyond that grade of development which is permanently characteristic of the marsupial Mammalia, the thalami being still incompletely covered in by them. During the fourth and

part of the fifth months, however, the middle lobes are developed from their posterior aspect, and cover the corpora quadrigemina; and the posterior lobes, of which there was no previous rudiment, subsequently begin to sprout from the back to the middle lobes, remaining separated from them, however, by a distinct furrow, even in the brain of the mature fœtus, and sometimes in that of older persons. In these and other particulars there is a very close correspondence between the progressive stages of development of the human cerebrum and those which we encounter in the ascending series of Mammalia. The phases of evolution of all parts of the higher organisms are represented by analogous and permanent conditions among the lower. In the organs of smell, for instance, the several steps of development are met with in the various classes of animals: the small closed fossæ remind us of fishes; the short nasal ducts opening into the anterior part of the mouth, of batrachians. A fine wool-like hair covers the human fœtus during the sixth month: the whole surface, including even the forehead and ears, is thus thickly clothed; but it is a significant fact that the palms of the hands and the soles of the feet are quite naked, like the inferior surfaces of all four extremities in most of the lower animals. As this can hardly be an accidental coincidence, the woolly covering of the fœtus probably represents the first permanent coat of hair in those mammals which are born hairy. The abnormal growths of hair occasionally observed in the human subject are similar in texture to the lanugo of the fœtus, and must be attributed to an arrest of development of the hair together with its continued growth.*

It is a noteworthy fact that, in cases of arrested development, the "blighted" part continues to grow independently of its stage of development.

The higher the organism, as a general rule, the more perfect is the state in which it enters the world: even the new-born human being is, however, far from fully developed; and Oscar Peschel remarks that the course of development of speech in tender years is approximately, if not completely, similar to the first attempts to speak made by our race.

A recent discovery of the Rev. W. Dallinger's has enabled us to trace the parallelism of descent and of individual development even further than from the commencement of embryonic evolution. He has seen a monad with

* Darwin, The Descent of Man.

flagellum (closely resembling a spermatozoon) absorb another monad prior to muliplication. This corresponds exactly with the union of the sperm-cell and the germ-cell in the fecundation of the ovum.*

Having briefly traced some of the reversions to lower types in the higher organisms, I shall now quote a few interesting notes on the individual development of the lower animals and of plants, from Oscar Schmidt's "Doctrine of Descent and Darwinism." He remarks that "The circumstances of parasitic worms are repeated by the parasitic Crustacea, as, moreover, a probably primordial form of the crab family is preserved in the metamorphoses of several orders of this large and diversified, though coherent, class. The larva which, it may safely be assumed, approximates closely to the primordial form, was at one time taken for an independent genus, and received the name of *Nauplius*. Hence a *Nauplius* phase is spoken of which obtains especially among the lower Crustacea, the *Copepoda*, parasitical Crustacea, and *Cirripedes*, and the remarkable *Rhizopoda* connected with them; but is not wanting in the highest order, the decapodous stalk-eyed crab. We shall later have to make acquaintance with the so-called curtailed development which among the crabs has been adopted by the decapods, and it was formerly supposed by all. Were this actually the case we should still, by analogy, infer their connection with the orders repeating the *Nauplius* phase in the course of their development; but it was a welcome discovery of Fritz Müller's that a shrimp (*Peneus*) still begins its development as a *Nauplius*, whereas all the other members of the order, as far as they are known, leave the egg in the higher *Zoea* phase. As of the hundreds of stalk-eyed crabs scarcely a dozen have hitherto been examined as to their development, it will not be doubted that, with regard to the *Nauplius* phase, some resemble the *Peneus* of the Brazilian coast. But even were this case to prove unique in the order, it would suffice as a living witness to the connection between the presence of the decapods and the primordial crabs. There can be no other view of the subject. The *Nauplius* phase in the development of the *Peneus* is either a shining testimony in favour of the doctrine of Descent or a senseless paradox." Again: "The shell of the individual Ammonite begins with the old morphological type, and then adopts the modifications in the same order in which they follow in vast periods in the

* This would seem to indicate that the distinction of sex is coeval with life itself.

geological development of the groups concerned." In another place he says—"The individual development of the *Cladomena*, and other *Medusæ* similarly propagated, corresponds with the systematic series of the *Medusa* polypes." He gives many other instances of parallelism of individual and racial development among the lower forms of life.

But the crowning fact of all is that the vegetable world conforms to the same law. A little further on, in the same chapter, Oscar Schmidt remarks—"Quite recently A. Braun has pointed out the accordance of the botanical system, and therewith of palæontological succession, with the development of the individual plant, when he says:—In the further elaboration of the Natural system, the gradation of the vegetal kingdom, and, at the same time, the relation of the system to the history of development, becomes more and more spontaneously and incontrovertibly manifest. The Acotyledons are verified as Cryptogams, as they were already considered by the old botanists of pre-Linnæan times, and their relation to the Phænogams is thus more clearly pronounced. The Cryptogams are separated into two essentially different divisions, in which gradation is likewise distinctly pronounced (cellular and vascular Cryptogams, Thallophytes, and Kormophytes); between the perfect Phænogams and the Cryptogams an intermediate grade has been shown, that of the Gymnosperms. But most important of all is the circumstance that the four chief grades ascertained in the vegetal kingdom accurately correspond with the grades of development occurring in the individuals of all the higher plants;—the germ, the vegetation stem, the blossom, and the fruit."

The parallelism of the metamorphoses of embryonic development, and the stages of historical evolution, is far more complete than the brief sketch I have just given of its more salient features would seem to convey. Its universality has, however, been sufficiently illustrated.

It is, then, a fundamental law of Nature that the development of each organism, independently, must recommence *ab initio*, and that the phases of the development correspond in every way with those of the evolution or expansion of the primordial germ from which it has sprung.

The first and main conclusion to be drawn from this law appears to be that the actual development of organisms is not due to Selection.

The second inference to be derived from it, in connection with other facts, is that organic development is not a mere manifestation of molecular force.

To elucidate the train of reasoning by which these deductions are arrived at, let us, for the sake of simplicity, consider the case of a hen's egg, in which we have two conditions of importance; the one consists in the protection of the embryo by the shell from all external influences: the other in the fact that it is completely isolated from the mother, and therefore that its development cannot be affected by any maternal formative power. Although the *fœtus in utero* is as little influenced by modifying causes, as the chick *in ovo*, the insect undergoing metamorphosis in the pupal stage, or the seed springing from the earth, it is not, in the first case, on a *primâ facie* view, so self-evident as in the latter ones. The conclusions we shall arrive at from the case of the chick will, it is evident, be nevertheless equally applicable by analogy to the whole of the animal and vegetal kingdoms.

When quite newly laid a hen's egg contains no trace whatever of the different systems, organs, and limbs which make their appearance in due order during the period of incubation, prior to which the contents of the egg are in a condition corresponding to protoplasm. Since the albuminous and other materials enclosed in the shell are protected by it from all extraneous modifying influences, the development of the chick (the various phases of which are precisely analogous both in their distinguishing traits, and in the order in which they occur, to those which have succeeded each other as characteristics of its progenitors), is undeniably effected without the aid of selection or any similar agent, but is due to a force within itself. Selection, therefore, is not necessary to development; but a development in every way analogous to that of the race is repeated in each individual by an agency which is not selection. The conclusion seems irresistible that the race was developed by the same agency. The conditions in the two cases are similar, the effects are uniform; are we not, therefore, justified in assuming that the cause in each is identical?

It seems to follow, then, that the phenomena of ontogenesis and those of phylogenesis are manifestations of one and the same developmental law—the law of correlative expansion.

It is worthy of notice that the earlier ancestral specific and generic modifications and adaptations, which undoubtedly are fairly attributable to natural selection, habits, &c., become obliterated and are gradually lost, when the developing agent acts free from extraneous influences, while only the leading types are retained. It is *superfluous* to have recourse to selection to account for racial development, when we know

that a faculty of evolution exists or can be set up in every organism. Embryonic development is inexplicable on the theory of selection, which cannot account for the recurrence of characteristics of which there is no trace in the parent form, or which belong to totally different species, genera, or families. For instance, it would be idle to ascribe to selection the reversion to the extreme limit of life observed in the spermatozoon. All we could expect in individual development, on the mechanical theory, would be an accurate replica of the parent form without the preliminary grades, that subsequently become effaced, and that have no reference to the advantage of the embryo, but are in general merely reversions to long-extinct progenitors. The developmental power, law, or principle, controlling influence, or whatever it be named, manifests itself in a multitude of organic phenomena. All features in an organism that cannot be attributed to selection must be referred to this influence. Serial and bilateral symmetry and type generally comes under this heading.

A law of correlative development is plainly indicated in the intimate connection and correlation of all parts of organisms, in the fact that no single character can be modified without a corresponding modification of all others; in short, in the bond of union that constitutes all parts of an organism, mathematically speaking, functions of one another and of the whole. This bond of union or correlation explains all cases of rudimentary growths, which must be regarded as necessarily coexistent with certain completely developed organs, limbs, or other portions of the structure, or with a certain stage or form of development of the organism, and not necessarily as parts in a state of transition. The mammæ of males and all secondary sexual characters owe their origin to correlative development or evolution; they are functions of the organs of generation, as it is well known that the male organs of generation are homologous with those of the female. It is impossible to explain this instance of rudiments in any other way, and it must therefore be regarded as a special case affording the strongest possible corroboration of the hypothesis of a developmental law. It is important to note that when the genital organs are abnormally developed and present features peculiar to both sexes in an imperfect condition, the secondary sexual characters also assume an intermediate state.

It is a significant fact, that where it is possible to decide to what progenitor the organism reverts in its embryo, it is generally found to be an extinct one, although closely allied

species are found still in existence. This would appear to indicate that the existing species have been evolved by the same law as the extinct ones, which have in reality become transformed into the higher type.

It is probable that many adaptations ascribed to natural selection may have been formed by instinct developed by a faculty of evolution or expansion, establishing habits which have reacted on the organisation. In this way it is possible to account for the incipient stages of modifications which cannot at first have been of any use to their possessors, and the difficulties urged by Mr. St. George Mivart on this subject may be thus disposed of. I have not made the nature of the developmental agency an element or condition in any of the arguments I have hitherto used. I have reserved this subject for the concluding part of the paper.

The question arises whether this agency be molecular force alone or not? Because heat accompanies all organic operations, it is inferred, by those who hold the mechanical view of life, that heat is the sole agent in all processes. To refer embryonic changes to the action of heat simply because they take place only between certain limits of temperature is as illogical and as unwarrantable as it would be to consider the shape of a horse-shoe due to the same cause, for no better reason than that the application of a certain amount of heat from the furnace is necessary to its formation. A quotation in the first part of this paper suggested that the mechanical view of life would be demonstrated only when all the motions in an organism are shown to be the effects of forces which at other times are inherent in its atoms. It could not be expected that atoms should lose their physical properties on becoming component parts of an organised structure; moreover, molecules do not at all times possess the same power of arranging themselves as when they become the building materials of a developing embryo.

It is certain that heat and light play an important part in the economy of life; it is probable that they perform all the *work;* but there is not sufficient reason for believing that they alone effect the *evolution* of organisms. There are, however, many reasons for believing the contrary.

We know that no imaginable combination of chemical operations, no possible application of heat or light, can produce one drop of an organic acid from inorganic materials.

Prof. Tyndall,* M. Pasteur, and the Rev. W. Dallinger,

* In a recent lecture of Professor Tyndall's, the following remarks occur:—"From the beginning to the end of the inquiry there is not, as you have seen,

have furnished us with excellent reasons for believing that between living and not-living matter there is no link, but, on the contrary, an impassable barrier; that inanimate matter could, by no combination of chances, become animate; in short, that this world, destitute of life-germs, would, through all the changes that have taken place during the ages that have elapsed since it first became habitable, have remained an absolute desert, without a vestige of even the lowest forms of life. Thus, although it may never be actually demonstrated, evidence is already forthcoming of such a nature as to render it in the highest degree probable that spontaneous generation even from dead *organic* matter, is impossible at the present day; and if impossible now, it can never have taken place at any period of the world's history, *physical laws being unalterable*. The inconsistencies of Profs. Huxley and Tyndall, on this point, have been clearly indicated by Dr. Bastian, who remarks that:—"We find Prof. Tyndall also affirming in the most unhesitating language the ultimate similarity between crystalline and living matter: affirming that all the various structures by which the two kinds of matter may be represented are equally the 'results of the free play of the forces of the atoms and molecules' entering into their composition. And yet he, too, would have us believe that whilst differences in degree of molecular complexity alone separate living from not-living matter, the physical agencies which freely occasion the growth of living matter are now incapable of causing its origination." In another place he comments thus on Prof. Huxley's views:—"What reason does Prof. Huxley give, in explanation of his supposition as to the present non-occurrence of Archebiosis? He says, if it were given him 'to look beyond the abyss of geologically recorded time' to a still more remote period of the earth's history, he would expect 'to be a witness to the evolution of living protoplasm from not living matter.' And the only reason distinctly implied why a similar process should not occur at the present day, is because the physical and chemical con-

a shadow of evidence in favour of the doctrine of spontaneous generation. There is, on the contrary, overwhelming evidence against it; but do not carry away with you the notion, sometimes erroneously ascribed to me, that I deem spontaneous generation impossible, or that I wish to limit the power of matter in relation to life. My views on this subject ought to be well known. But possibility is one thing and proof is another; and when in our day I seek for experimental evidence of the transformation of non-living into the living, I am led inexorably to the conclusion that no such evidence exists, and that in the lowest, as in the highest, of organised creatures, the method of nature is that life shall be the issue of antecedent life."

ditions of the earth's surface were different in the past from what they are now. And yet, concerning the exact nature of these differences, or the degree in which the different sets of conditions would respectively favour the occurrence or arrest of an evolution of living matter, Prof. Huxley cannot possess even the vaguest knowledge. He chooses to assume that the unknown condition existing in the past were more favourable to Archebiosis than those now in operation. This, however, is a mere assumption which may be entirely opposed to facts. It is useless, of course, to argue upon such a subject, but still it might fairly be said, in opposition to his view of the impotency of present telluric conditions, that the abundance of *dead organic matter* now existing in a state of solution would seem to afford a much more easy starting-point for life-evolution than could have existed in that remote past, when no living matter had previously been formed, and consequently when no dead organic matter thence derived could have been diffused over the earth's surface. (This is a consideration of great importance; since those who believe that Archebiosis occurs in *organic* solutions at the present day have not yet professed to show that it can occur in *saline* solutions free from traces of organic matter.)

"Professor Huxley is, however, very inconsistent, since, in spite of his declared expectation of witnessing the evolution of living from lifeless matter, if it were given him to look beyond the abyss of geologically recorded time, he had said scarcely five minutes before, in reference to experimental evidence bearing upon the present occurrence of a similar process, that 'if, in the present state of Science, the alternative is offered us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to re-arrange themselves in living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be, even for a moment, doubtful.'"

The inconsistency pointed out by Dr. Bastian is undoubtedly very apparent. Whether Professors Tyndall and Huxley accept the law of Archebiosis for the past and reject it for the present, or, *vice versa*, they are equally inconsistent. Spontaneous generation is either a law of nature, or it is not. Imaginary telluric conditions in the ages that preceded geologically recorded time could not affect the question, *which must depend on the immutable properties of matter*. To those who do not believe in the present occurrence of

Archebiosis, there is but one legitimate course open—the rejection of the theory *in toto*. The subject being still *sub judice*, we do not insist on the impossibility of spontaneous generation. Even were it shown that the spontaneous generation of living things did occur at the present day from *inorganic matter*, it would merely lead us to believe that the organic developmental agency lies latent until called into activity by special conditions, *but not that it is identical with crystalline force*.

Professor Huxley remarks "that the property of crystallising is to crystallisable matter what the vital property is to albuminoid matter." But this analogy is not supported by facts. Crystallisable matter can never have crystallised in any other forms than those it assumes at present, and it must continue to crystallise in one set of forms only for all time; it can never develop into new shapes, but shall remain changeless for ever. Protoplasm, on the contrary, has, from the first moment it became active, never ceased to develop into countless varieties of organic structures, and is still developing into ever new and higher forms. While the distinguishing attribute of crystallisable matter is unchangeableness, that of albuminoid matter is continual progress. The action of the molecular forces on inorganic matter, under given conditions, can be predicted with the precision of abstract mathematics; but who shall foretell the forms of future living beings?

It seems highly probable, that in all organic matter there exists a controlling principle, directing the action of the molecular forces.

The organic machine is set in motion by heat, but its movements are regulated by a rigid law; it is constructed to perform a prescribed series of operations only. *The development of the germ may thus be regarded as due to self-acting and self-adapting mechanism of inconceivable refinement.* We find no force at work but molecular force, yet we observe phenomena that cannot be referred to the known properties of inorganic matter. We are therefore compelled (as the only apparent alternative) to believe in the automatic action of the forces we recognise; or in other words, that the development of organisms is due to a necessary sequence of operations, which must have for basis or starting point a certain definite and regular disposition of cells or molecules in the primordial germ; or, given protoplasm in a certain condition, development is effected by the laws of inorganic matter. This view of Evolution appears to negative the theory of Spontaneous Generation.

The fact already alluded to, that the development of an organism may be arrested either wholly or partially, and its growth continue as if no such arrest had taken place, has an important bearing on this subject, for it appears to show that development is distinct from the nutritive and reparative—in short, the purely mechanical processes of life—and that it is therefore due to an additional cause, the action of which may be suspended without arresting the ordinary physical processes.

The correlative development of all parts of an organism already referred to evinces systematic evolution. The convergence of all types to one, and the existence at the present day of forms closely allied to others long extinct, appears to indicate that organisms admit of development or evolution by certain invariable metamorphoses only.

We have no longer, at the present day, to concern ourselves with establishing the Evolution Hypothesis. Almost all those who are in a position to form a judgment are agreed in accepting it.

We must now confine ourselves to making a choice between two theories in explanation of the doctrine of descent. *First*, we may follow the Darwinians and believe that natural and sexual selection, habits, and other accidental circumstances are sufficient to account for all organic phenomena. If we maintain this, we are bound also to admit the purely mechanical view of life; because, in asserting the all-sufficiency of selection, &c., we preclude the action of all other agents with the exception of molecular force.

Again, if we adopt the mechanical view, we must, to be consistent, consider natural selection, &c., adequate to account for all forms of structure and all organic changes, because we eliminate all other possible agents—crystalline force, from its very nature, being wholly blind, casual, accidental, and in every way purely fortuitous. Furthermore, those who hold mechanical causes, directed by natural selection, &c., sufficient to account for the evolution of all animals and plants, are logically compelled to admit spontaneous generation as a necessary consequence; for otherwise there is imposed an arbitrary limitation to the extent or range of power of the molecular forces, and it is admitted that a first step is necessary, after which mechanical causes come into play—this is tantamount to acknowledging the existence of an agent separate and distinct from mechanical action. Thus the theory of selection, as sole agent of evolution, the mechanical view of life, and the theory of spontaneous generation, are inseparable; *they must stand or fall*

together, if one be proved invalid, all three are untenable. This position cannot be too strongly insisted on. Fully realised it would prevent much fruitless discussion. So-called Darwinianism is thus assailable on three points of great importance. Absolute proof or disproof of any of these is perhaps impossible, *but probability may be increased indefinitely in one direction or the other, so as virtually to amount to a certainty.*

Secondly, we may believe in a necessary and predestined sequence of molecular operations constituting a developmental law—assign to selection its legitimate work, modification, and adaptation, and reserve any expression of opinion as to the origin of life.

The question thus narrows itself to this: have organisms been developed by the selection of purely fortuitous aggregations of atoms with no other bond of union than reciprocal attraction and repulsion; or have they been systematically evolved by a pre-existent law? There seems to be but one difficulty in the latter suggestion—the origin of life; and this difficulty presents the only reason for accepting the first. Still it is possible that the power of organic development may have lain latent in inorganic matter since the universe itself was a germ—a primal haze—until such time as existing conditions favoured the beginning of life; but it seems more probable that protoplasm is coeval with inorganic matter, and that it primarily existed in what, for want of a better term, we may call a dormant condition. It has been fancifully and somewhat practically suggested* that the germs of life in our world may have sprung from some moss-grown fragment hurled from the ruins of another planet. This would, of course, be merely transferring the difficulty to some far-off world in which the origin of life must be as profound a mystery as it is in ours. In striving to find the truth we must not accept apparent possibilities, *or take refuge in illusory speculations as to the past condition of the earth.* We must divest ourselves of all preconceived ideas whatsoever, religious or scientific. We must not occupy ourselves too much with details, or special instances; but we must check our conclusions by the fundamental laws of nature.

The unfolding through unmeasured ages of the lowliest primordial forms, and their expansion to ever new and higher structures, resulting in the vast ascending series of plants and animals—the one with its interminable succession

* By Sir William Thomson.

of graceful forms and brilliant hues (forms and hues which it would be idle, even ridiculous to ascribe to utilitarian causes); the other, with its endless varieties of habits and instincts, its innumerable phases of organs, limbs, and systems, in which type is ever persistent and dominant, with its gradually increasing complexity of mechanism (in the contemplation of which the mind is unable to divest itself of the idea of design); all this, and more—the human intellect, human affections and aspirations—we must believe to be the results of *only a mode of motion*, or else we must acknowledge the presence of an influence the nature of which we can at presence but faintly attempt to conceive.

If we accept this latter view of evolution, the speculations of Mr. Wallace, and the views of Mr. Darwin, which Oscar Schmidt appears to consider regrettable, are seen to be the incomplete expression of, perhaps, the noblest conception of the nineteenth century.

NOTICES OF BOOKS.

The Effects of Cross- and Self-Fertilisation in the Vegetable Kingdom. By CHARLES DARWIN, F.R.S. London: John Murray.

THAT this work is intended as a contribution to the body of cumulative evidence already collected by Mr. Darwin in support of his views on the origin of species need not be questioned. But consisting as it does not of generalisation or controversial matter, but of the records of careful experiments and observations, it has a value quite independent of theories. Should hereafter the hypothesis of "natural selection" be superseded, should the doctrine of Evolution in any shape be abandoned, the volume before us must still remain a highly important contribution to biological science.

The author on the threshold of his enquiry points to the abundant evidence that flowers are constructed so as to be cross-fertilised, occasionally or habitually, by pollen from another flower, whether growing on the same or on a different plant. To ensure such cross-fertilisation a number of curious arrangements exist, which the author and other observers have elsewhere described, and to which he only therefore refers in passing. We will merely remind the reader that this purpose is in some cases secured by a separation of the sexes of flowers, whilst in others the pollen and the stigma of the same flower are not matured at the same time. Sometimes the impregnation of flowers by their own pollen is prevented, or at least impeded, by beautiful mechanical contrivances. In one class the ovules "absolutely refuse to be fertilised by pollen from the same plant, but can be fertilised by pollen from any other individual of the same species."

Mr. Darwin's present concern is not with the means, but with the ends of cross-fertilisation. It would be "simpler," surely, for every plant to have been fecundated by its own pollen; but finding this state of things in a number of cases so carefully guarded against, we are warranted alike on the principles of the Old and the New School of Natural History in supposing that we have before us no mere accident. The author was led to undertake the experiments hereinafter detailed by the following circumstance:—For the sake of determining certain points with respect to inheritance, and without any thought of the effects of close inter-breeding, I raised close together two large beds of self-fertilised and crossed seedlings from the same plant of *Linaria vulgaris*. To my surprise the crossed plants, when

fully grown, were plainly taller and more vigorous than the self-fertilised ones." Mr. Darwin, however, whom some persons accuse of rushing hastily to conclusions without sufficient evidence, considered it still "quite incredible that the difference between the two beds of seedlings could have been due to a single act of self-fertilisation." The next year he performed an analogous experiment. "I raised, for the same purpose as before, two large beds close together of self-fertilised and crossed seedlings from the carnation, *Dianthus caryophyllus*. This plant, like the *Linaria*, is almost sterile if insects are excluded, and we may draw the same inference as before, namely, that the parent-plants must have been inter-crossed during every—or almost every—previous generation. Nevertheless, the self-fertilised seedlings were plainly inferior in height and vigour to the crossed."

A formal series of experiments was then undertaken with various plants, and was continued for eleven years, the crossed plants in the great majority of cases being found to have the advantage. The general mode of experimentation was as follows:—"A single plant, if it produced a sufficiency of flowers, or two or three plants were placed under a net stretched on a frame, and large enough to cover the plant without touching it. This latter point is important, for if the flowers touch the net they may be cross-fertilised by bees. I used at first white-cotton net with very fine meshes, but afterwards a kind of net with meshes one-tenth of an inch in diameter. On the plants thus protected several flowers were marked, and were fertilised with their own pollen; and an equal number on the same plant, marked in a different manner, were at the same time crossed with pollen from a distinct plant. The crossed flowers were never castrated, in order to make the experiments as like as possible to what occurs in Nature with plants fertilised by the aid of insects. In some few cases of spontaneously self-fertile species the flowers were allowed to fertilise themseves under the net, and in still fewer cases uncovered plants were allowed to be freely crossed by the insects which incessantly visited them."

The seeds from the flowers thus treated were allowed to ripen thoroughly, and were then allowed to germinate, with the following precautions:—"The crossed and self-fertilised seeds were placed on damp sand, on opposite sides of a glass tumbler covered by a glass plate, with a partition between the two lots, and the glass was placed on the chimney-piece in a warm room. I could thus observe the germination of the seeds. Sometimes a few would germinate on one side before any on the other, and such were thrown away. But as often as a pair germinated at the same time they were planted on opposite sides of a pot, with a superficial partition between the two; and I then proceeded until from half-a-dozen to a score or more seedlings, of exactly the same age, were planted on the opposite sides of several pots.

If one of the young seedlings became sickly or was in any way injured it was pulled up and thrown away, as well as its antagonist on the opposite side of the same pot.

"As a large number of seeds were placed on the sand to germinate, many remained after the pairs had been selected; these were soon crowded together on the opposite sides of one or two rather large pots, or sometimes in two long rows out of doors. In these cases there was the most severe struggle for life among the crossed seedlings on one side of the pot and the self-fertilised seedlings on the other side, and between the two lots which grew in competition in the same pot. A vast number soon perished, and the tallest of the survivors on both sides when fully grown were measured. Plants treated in this manner were subjected to nearly the same conditions as those growing in a state of Nature which have to struggle to maturity in the midst of a host of competitors."

Sometimes the seeds, instead of being previously allowed to germinate on damp sand, were sown at once on opposite sides of pots, and the plants measured when fully grown. This plan Mr. Darwin pronounces less accurate, as the seeds sometimes germinated more quickly on one side than the other. He considers, however, that it was necessary thus to proceed in the case of some few species, "as certain kinds of seeds would not germinate well when exposed to the light." We should suggest that such seeds might have been covered with a plate of blue glass, since blue light—though not favourable to plants in the later stages of their life—undoubtedly promotes germination.

Every precaution was taken that the two classes of seedlings under comparison should in all other respects be precisely on an equality. The soil was evenly and thoroughly mixed, the supply of water and the exposure to light were the same. Yet, as we have already intimated, the self-fertilised plants, when carefully weighed and measured, were decidedly inferior to the crossed.

Hence Mr. Darwin is perfectly justified in the inference that cross-fertilisation is generally beneficial and self-fertilisation injurious. "That certain plants," he remarks, "such as *Cyclamen persicum*, &c., which have been naturally cross-fertilised for many or all previous generations, should suffer to an extreme degree from a single act of self-fertilisation is a most surprising fact. Nothing of the kind has been observed in our domestic animals; but then we must remember that the closest possible interbreeding between such animals—that is, between brothers and sisters—cannot be considered as nearly so close a union as that between the pollen and the ovules of the same flower. Whether the evil from self-fertilisation goes on increasing during successive generations is not as yet known, but we may infer from my experience that the increase, if any, is far from rapid. After plants have been propagated by self-fertilisation for several generations, a single cross with a fresh stock restores their pristine

vigour; and we have a strictly analogous result with our domestic animals."

The question whether a vegetable species can be reproduced asexually, *i.e.*, by rhizomes, stolons, &c., from a very remote period remains open. Andrew Knight maintained that a variety exclusively thus propagated, like the majority of our fruit trees, must ultimately become weakly, and Prof. Asa Gray leans to the same view. It would be interesting, if the Anti-Vivisectionists would allow it, to take some animal capable of propagation by "cuttings," and try for how many generations this mode of reproduction could be carried on without a visible decay of vigour.

Mr. Darwin guards against the inference that cross-fertilisation is, *per se*, beneficial under all circumstances. His experiments show that the "benefit from cross-fertilisation depends on the plants which are crossed having been subjected during previous generations to somewhat different conditions." Thus plants which had been self-fertilised for the eight previous generations were crossed with plants which had been inter-crossed for the same number of generations, all having been kept under the same conditions as far as possible; seedlings from this cross were grown in competition with others derived from the same self-fertilised mother-plant crossed by a fresh stock, and the latter seedlings were to the former in height as 100 : 52, and in fertility as 100 : 4." The advantages of a cross, Mr. Darwin considers, "depend altogether on the differentiation of the sexual elements, a conclusion which harmonises perfectly with the fact that a slight and occasional change in the conditions of life is beneficial to all plants and animals. We thus see that in many species plants fertilised with their own pollen are either absolutely sterile or very sparingly fruitful; if fecundated with pollen from another flower on the same plant, they are sometimes a little more fertile; if treated with pollen from another individual or variety of the same species, their fertility is at its maximum; but if with pollen from a different species their fertility declines, till we arrive at absolute sterility. "We have thus a long series with utter sterility at the two ends; at one end due to the sexual elements not having been sufficiently differentiated, and at the other end to their having been differentiated in too great a degree or in some peculiar manner." But having penetrated so far we must confess our ignorance. "We do not know what is the nature or degree of the differentiation in the sexual elements which is favourable for union, and what is injurious." Some species are greatly benefitted by crossing, while others profit very little. Some plants retain their vigour after having been self-fertilised for untold generations. But for these and for many connected facts we can scarcely conjecture a reason.

Going still further, and admitting—as we are compelled—that fertile eggs can be produced without the co-operation of the male,

we ask, why have the two sexes been developed? Mr. Darwin finds the answer in the fact that the offspring of two distinct parents, especially if descended from stocks exposed to somewhat dissimilar conditions, have an advantage in vigour over the progeny of a single self-fertilised individual. But if, as appears probable, the sexes were primordially separate, why did they become blended into hermaphrodite forms, and why—in all the higher animals and in some plants—have the sexes again been separated? The bilateral structure of animals, as Mr. Darwin suggests, perhaps indicates that they were aboriginally formed by the fusion of two individuals. In connection with this subject we have had occasion to refer to certain curious cases of bilateral hermaphroditism found among moths, where one wing, antenna, &c., bear the characters of the male, whilst the other side is as plainly female. But we have vainly sought for any analogous instance either among other insects or among birds and mammals.

The whole tendency of these researches, when calmly and impartially weighed, must be to shake the confidence commonly felt in the primordially distinct character of "species" as compared with mere varieties. The difference in the affinities of the sexual elements of different species, on which their mutual incapacity for breeding together depends, is caused by their having been habituated for a very long period each to its own conditions and to the sexual elements having thus acquired firmly fixed affinities."

The Various Contrivances by which Orchids are Fertilised by Insects. By CHARLES DARWIN, F.R.S. Second Edition, Revised. London: John Murray.

THIS work is already too widely and too favourably known to require examination or comment. The present edition has been, as the author informs us, enriched with many new and curious facts communicated by correspondents in different parts of the world, among whom especial mention is made of Dr. Fritz Müller. A few errors have also been corrected. We cannot help regarding it as a somewhat unfortunate omission that the author has not given a list of the additions and modifications introduced into the present edition. It must, however, be distinctly understood that the alterations thus made are far from invalidating the conclusions reached in the former edition. A list is appended of all the memoirs and books bearing on the fertilisation of the Orchideæ which have appeared since the first appearance of the present work, in 1862. It is somewhat singular that, whilst the botanists of England, America, Germany, and Italy have laboured diligently in the investigation of this

interesting question, those of France still hold aloof, and contribute nothing towards the solution of the problems here stated or suggested. For instance, it may well be asked why, in spite of all the wonderful contrivances for fertilisation which we are compelled to recognise, so few of the seeds of the Orchids are really productive? According to Mr. Scott a single plant of an *Acropera* may sometimes yield seventy-four millions of seeds. In a single capsule of a *Maxillaria* Fritz Müller found 1,756,440 seeds. Yet some unknown cause checks their multiplication, so that, despite the astonishing number of their seeds, they are as a rule sparingly distributed. In no country is the number of individuals of any one species nearly so great as that of very many other and far less prolific plants.

The following fact deserves to be seriously considered by all who are engaged in experimenting on the part played by insects in the fertilisation of plants. According, namely, to Mr. Moggridge, "*Ophrys scolopax* fertilises itself freely in one district of Southern France without the aid of insects, and is completely sterile without such aid in another district."

The seventh chapter of the book, treating of the fertilisation of the *Catasetidæ*, is strangely suggestive. The flowers of the male plant, if touched at certain definite points by an insect, shoot forth their pollinia, which, being furnished with excessively adhesive points, cling to the intruder, and are by him carried to a female plant. If this is mere automatism, where in the organic world are we to draw a sharp boundary line between such mere mechanical action and the "instinctive" performances of the lower animals, or even of man? But if there be nothing automatic in the latter, can we venture to deny that the plant may also have its instincts, and even its dim self-consciousness? What if the old myth of the hamadryads foreshadowed a great truth?

Through Norway with Ladies. By W. Mattieu Williams, F.R.A.S., F.C.S., &c. London: E. Stanford.

The mere title of this book will at once reveal its origin and secure for it a favourable reception. Nor will such persons as are led to take up the work, by their pleasant recollections of its companion volume, find themselves disappointed. Mr. Williams journeys not to have "done" certain localities, but to see and to learn: he finds interesting facts which too many ramblers overlook; he draws valuable lessons from phenomena apparently threadbare, and he lays before us his observations and reflections in a genial and pleasing manner, and without "posing" for our admiration. If the work before us does not equal certain records of travel which have appeared within the last quarter of a

century, the cause must be sought out not so much in any shortcoming of the author as in the nature of his subject. The *fauna* and *flora* of Norway are comparatively poor, and differ little from those of the rest of Northern Europe, and its natural phenomena in general have been well and often investigated. That Mr. Williams has been able to secure such valuable gleanings from so ably reaped a field makes us regret the more that he has not turned his attention in preference to some richer and less-known region. But his tastes are evidently ultra-Hyperborean; he advocates "Arctic Expeditions for the Million," and declares that he should hugely enjoy an excursion up to 81° 30′ N. lat., the bare idea of which causes our flesh to creep.

Among the abundance of interesting matter it is not easy to make a selection. So plunging at once *in medias res* we are reminded that Norway is the classical home of the sea-serpent. No scientific traveller can visit the fjords without attempting to solve this mystery and to account for its alleged existence. Our author's conjecture, though we cannot accept it, must be pronounced bold and original. Observing the existence of certain low interrupted ridges of rock stretching out into the sea, he thinks that these, under certain atmospheric conditions, may have been mistaken for the coils of an enormous serpent, "floating many a rood." It must, of course, be admitted that the currents of air which in bright sunny weather may be perceived rising up from a heated surface may give a semblance of motion to a fixed and inanimate object. It will also be granted that, according to Pontoppidan and other the like authorities, the sea-serpent was in the habit of appearing only in calm, sunny weather, in the height of summer, and of sinking down out of sight as soon as a slight wind happened to spring up. So far the features of the case are in favour of our author. But his supposition would, at best, only account for the appearance of the supposed monster in some very few localities. Further, we cannot go so far as to assume that fishermen—familiar, from their youth up, with the existence of the rocks in question and with their aspect in every kind of weather—could be thus easily deceived. Suppose the sun to become overcast, or a wind to spring up, the undulations of the air would of course cease, and the seemingly life-like movements would be at an end. But the rocks would remain in their wonted places, and the spectators—instead of concluding that a serpent previously present, in addition to these rocks, had just disappeared—would far more probably draw the correct inference, that they had mistaken a motionless ridge for a living creature.

Let it not be for a moment supposed that we hold a brief for the sea-serpent. The existence of a marine Ophidian surpassing the python and the boa in size, in the same proportion as the whale exceeds the elephhant, has become too improbable to be

established by any evidence short of its actual capture and exhibition. Were we to have an opportunity of observing the supposed monster, even at a very short distance, we should keep complete silence on the interview. But Mr. Williams's explanation seems to us as little satisfactory as the hypotheses of floating barrels, timbers of a wrecked vessel, with or without sea-weed attached, gigantic seals, sharks, &c., which have been put forward by other writers. It is simpler and safer to explain the alleged phenomena by the aid of "unconscious cerebration." The observers, whose "early scientific education" had probably in all cases been neglected, have allowed themselves to become the slaves of a "DOMINANT IDEA."

On the colour of water we find some interesting remarks. The author considers that "after deducting the influence of the reflected light of the sky or the clouds, the residual special colour is *directly* due to the minute particles suspended in the water." As instances of the colourless nature of very pure, even though deep, waters, he mentions the Aachensee in the Tyrol, and the Anapo near Syracuse. "Floating in a boat over the fountain of Cyane, the source of the Anapo, and looking down at the pebbles, seen with microscopic distinctness 40 or 50 feet below, was suggestive of sitting in the car of a balloon." In comparison with these waters he considers the "green Rhine and the blue Rhone" both lacking in transparency, their different shades of colour being due to suspended rock particles. The deepest purple or indigo waters he has found in the neighbourhood of dark slaty rocks. In limestone districts, and amidst red and deep yellow sandstones, the waters are green. The waves of the Atlantic along the west coast of Ireland are "more deeply indigo in tint than out at sea," owing to a vast quantity of suspended dark purple rock particles, too small to be separately visible, even with the aid of the microscope. Waters flowing through peat bogs are brownish when shallow, but pitchy black when deep, and especially when seen in the shade. This colour the author believes is actually due to a kind of pitch derived from the peat.

With reference to the profusion of cherries, currants, and gooseberries in Norway, Mr. Williams seeks the cause in the absence of sparrows and other strong-billed birds. Hence he dissents from those writers who denounce sparrow-clubs, and he defends the English farmer against the charge of being an ignorant and indiscriminating murderer of all small birds. To a great extent he is here in the right: the exportation of sparrows to Australia was a folly and a crime only one degree smaller than the introduction of rabbits, goats, and swine into uninhabited islands, or the proposal to supply a "constitutional check" to the first of these three destructive animals by the importation of polecats. The chief difficulty of the case is the impracticability of at once extirpating the sparrows and preserving the insect-

eating birds. The decrease of many insectivorous species is due not so much to any act of the farmers as to nest-hunting boys, holiday sportsmen, and to those professional bird-catchers who are found in all our large towns, and who, in utter defiance of the statute decreeing a "close time" for small birds, extend their ravages for fifty miles around London.

If a digression may be here permitted, we are informed that the starling—a purely insectivorous and most useful bird, and one which Mr. Williams will find eats larvæ not merely when hard pressed for food—is debarred from even the doubtful protection of the Wild Birds Protection Act. The reason assigned is, that it is sometimes selected as the victim of those eminently British institutions, gun-clubs. In this humanitarian, anti-vivisectionist, and moreover thoroughly practical country, the infliction of pain and death—even in defiance of utilitarian considerations—is a sacred institution if wanton amusement is the object; but if valuable knowledge is sought for the deed becomes an "orgy of diabolism."

Like many other travellers, from the time of Linnæus downwards, Mr. Williams was annoyed by the musquitoes of Lapland, which are here no less numerous than in any tropical swamp. To explain their abundance in these high latitudes he points to the total absence of swallows. That these birds do contribute powerfully towards the reduction of such minute vermin is undoubted; but musquitoes, unfortunately, are quite able to co-exist with swallows; indeed, varying in name and in trifling morphological details much more than in malignity, they infest upwards of three-fourths of the land on this our globe, and afford to the teleologist and the optimist a riddle which has not yet found its Œdipus.

Our author's main object, in both his visits to Norway, appears to have been the study of the so-called Glacial epoch. On this subject he has reached conclusions different from those formed by certain other geologists. He does not at all question the former "existence of two or more such epochs during the later period of the Tertiary age, with an intervening warm period or periods." But he considers that the magnitude of the phenomena has been greatly exaggerated. He quotes from Mr. Geikie's "Great Ice Age" (p. 561) a passage which we may here reproduce for the convenience of the reader:—"All Northern Europe and Northern America disappeared under a thick crust of ice and snow, and the glaciers of such regions as Switzerland assumed gigantic proportions. This great sheet of land ice levelled up the valleys of Britain, and stretched across our mountains and hills down to low latitudes in England. Being only one connected or confluent series of mighty glaciers, the ice crept ever downwards and outwards from the mountains, following the direction of the principal valleys, and pushing far out to sea, where it terminated at last in deep water, many miles

away from what now forms the coast-line of our country. *This sea of ice was of such extent that the glaciers of Scandinavia coalesced with those of Scotland and the north-eastern districts of England, upon what is now the floor of the shallow North Sea,* while a mighty stream of ice, flowing outwards from the western sea-board, obliterated the Hebrides, and sent its icebergs adrift in the deep waters of the Atlantic."

In Mr. Williams's opinion the configuration of the Lofodens decidedly contradicts the passage put in italics. The glaciers swept over the inner portion of this group, as is proved by the rounded, "hog's-back" form of the small rocky islets between Saltenfjord and Röst, but thinned out to seaward and failed to reach the rocks of Röst, which show no marks of the grinding-down action of ice-masses. Hence the author infers that the glaciers were insufficient to push fifty miles out to sea, "unless we suppose the submergence of the land to have been so great that the out-thrust *mer de glace* floated over these low rocks without grazing them." He concludes that "*even at the bitterest period of greatest European glaciation the waters of the ocean within the Arctic circle were warm enough to thaw, with considerable rapidity, the ice which had accumulated on the mountains and in the valleys of the land.*"

The aspect of the North Cape and of the other headland along the northern face of Europe seems also, to the author, to refute the idea of a great ice-sheet having "proceeded radially from the north polar regions, and overswept Scandinavia and the rest of Northern Europe." He finds that the "craggy headlands and the structure all indicate that the ice-sheet moved in the contrary direction, from south to north." It may, he admits, be urged in opposition that weathering has produced the precipitous character of the headlands since the Glacial epoch. This, however, he argues is contradicted by the "absence of a sea-beach such as would be formed by the material washed down by the waves had they done the great amount of work necessary for the conversion of glaciated slopes into precipices rising above 1000 feet out of the water." The till he considers to have been deposited from the under surface of glaciers not resting upon the ground, but floating in shallow water. The difficulty of the ground moraine theory, he thinks, is that of reconciling the existence of such a deposit as the till "with the tremendous erosive power of the glacier moving over it; especially when we consider that at the time of its deposit from the thawing ice, the matrix, the present stiff clay must have been a *purée* of thin slimy mud."

In the ice-age he holds that the seas adjacent must have been much deeper than at present. Not only would the earth's centre of gravity be, he contends, affected by the piled-up masses of ice and snow, but these very masses must, by their own gravitation, raise the nearest sea-level. He quotes Prof. Ramsay to the effect that the "probable submergence during some part of the

Glacial epoch may be taken at about 2300 feet." Mr. Belt, on the other hand, holding that the whole globe was glaciated not by alternate hemispheres, but simultaneously, considers that the amount of water abstracted from the ocean, and piled up in a solid form upon the continents, must have been so considerable as to reduce the general sea-level and lay bare lands now submerged to depths ranging from 1000 to 2000 feet, which would afford a temporary asylum for the tropical flora and fauna.

Mr. Williams seeks the cause of glacial epochs simply in alternating lengths of summer and winter in the two hemispheres. He holds that "no shifting of the earth's axis, no extreme convulsion, is demanded; for if, under the present distribution of terrestrial climate, the southern hemisphere had as much land around its poles as the northern hemisphere now has, it would be glaciated from the South Pole down to the latitude corresponding to that of London." It appears to him that "if we simply add the changes of climate which, as Lyell has shown, must of necessity result from variations in the distribution of land and water to the similar changes which must follow as necessary results of the known and demonstrable variations of the earth's orbit, we shall have in this combination an agency of sufficient energy to explain all the known phenomena." In his former work, "Through Norway with a Knapsack," Mr. Williams has contended that "low temperature is only one of the factors in the formation of glaciers, and that an increase of atmospheric humidity, and of consequent winter snow-fall, may produce the same effect as lowering of temperature." He supposes, indeed, that in the ice-age "the temperature of the northern hemisphere was lower than at present, but not nearly so low as is commonly supposed; and that this moderate depression of temperature, combined with a greater snowfall in winter, produced all the observed results. The lower temperature would favour increased precipitation, and the increased humidity of the air would resist the passage of the solar rays and greatly diminish the summer thawing."

According to Mr. Williams, then, the southern hemisphere is now "enjoying" its glacial epoch, though not in a very aggravated form. But it is possible, or rather highly probable, that an agency which he leaves out of account may very much modify the character of these ice-ages. The orbit of the earth varies periodically, alternately approaching nearer to a circle, and then becoming more eccentrically elliptical. This cause, as Mr. Croll has shown, will sometimes greatly intensify the influence of the shortened summer, and may thus produce epochs of a more intensified glaciation.

There are other subjects, not a few, on which our author expresses himself in an original and interesting manner, but they are not adapted for discussion in the "Quarterly Journal of Science." We will therefore conclude by thanking him for the

pleasure and instruction we have derived from the perusal of his book, expressing our conviction that most of his readers will have occasion to pay him a similar tribute.

The Amateur Mechanics' Practical Handbook. By Arthur H. G. Hobson. London: Longmans and Co.

This little manual treats, in successive chapters, on the lathe and its uses; on the drilling and planing machine; on the vice, bench, and hand-tools; on drawing and pattern-making; on the brass furnace and moulding; on the construction of horizontal engines; and on boilers. The author declares that his object has been to give plain, practical instructions, avoiding intricate subjects. Previous works which have dealt more or less prominently with this subject give, he finds, very vague instructions, or else are too theoretical to be of much use to the amateur—charges which we think are not unfounded. The directions contained in this little volume are full and precise, and are illustrated with numerous figures. We believe that Mr. Hobson has, by the publication of this treatise, conferred a boon upon amateur mechanics—a somewhat numerous class.

The Elements of Machine Design: an Introduction to the Principles which Determine the Arrangement and Proportions of the Parts of Machines, and a Collection of Rules for Machine Design. By W. Cawthorne Unwin, Professor of Hydraulic and Mechanical Engineering at the Royal Indian Civil Engineering College. London: Longmans and Co.

This work belongs to the series of "Text-Books of Science" issued by Messrs. Longmans, though by the strict methodologist it would be pronounced as belonging to the domain not of *science*, but of *art*—a distinction too commonly lost sight of, and, if we may judge from the last sentence of the Preface, not fully comprehended by the author.

In the first chapter Prof. Unwin treats of the materials used in the construction of machines. Here the properties of the various kinds of cast- and of wrought-iron are carefully explained. Phosphor-bronze is noticed at some length, the author expressing the opinion that it is likely to be of great service in machine construction. Its chief drawback in the eyes of the chemist is that its manufacture withdraws a portion of phosphorus from natural circulation. Might not an alloy of similar properties be obtained by substituting arsenic for phosphorus?

In successive chapters the author considers the straining action to which machines are subject; the strength of materials, fastenings, pipes, and cylinders; of pivots, axles, and shafting; of bearings for rotating-pieces; of toothed, belt, and rope gearing; of link-work; and of pistons, valves, and cocks.

The work may be fairly pronounced to have no small share of the invaluable attribute of thoroughness, and we think that it is very well calculated to effect its primary object. This, as the author tells us, is "to explain the principles that are available as guides in machine-construction. So far as it succeeds in this it will place the draughtsman in the best position to make use of the facts which come under his notice in the workshops." We must call particular attention to these last few words, because they seem to us to summarise the whole purpose of technical education. Every artizan ought to be so trained that he may make use of such facts. To this end he must be taught to observe, in order that the facts may not altogether escape him, and he must be acquainted with the scientific principles on which his art is based, in order that he may know how to deal with such facts.

The Ancient Life-History of the Earth, a Comprehensive Outline of the Principles and Leading Facts of Palæontological Science. By H. Alleyne Nicholson, M.D., D.Sc. Edinburgh and London: Blackwood and Sons.

The especial object of the work before us, as we are told in the Preface, is the study of fossil animals from the historical point of view, regarding them "principally as so many landmarks in the ancient records of the world," and examining "their relations to the chronological succession of the strata in which they are entombed." Their morphology and their relation to existing forms of life occupy here a mere subordinate place.

This preliminary demarcation of his subject having been drawn, the author enters upon the consideration of the laws of geological action. Happily observing that, in its strict sense, "Geology is nothing more than the Physical Geography of the past, just as Physical Geography is the Geology of to-day," he discusses, in a most philosophical spirit, the two grand conflicting theories of Catastrophism and Uniformitarianism. Here, whilst insisting that the present state of our earth is "really the result of the tranquil and regulated action of known forces through unnumbered and innumerable centuries,"—that "the history of the earth has been one of *law* in all past time, as it is now,"—that the successive groups of animals and plants revealed to us in turning over the pages of the "stone book" are, "to a greater or less extent, directly connected with one another, each being

the lineal descendant of the group which immediately preceded it in point of time," and being "more or less fully concerned with giving origin to the group which immediately follows it,"—he yet reminds us that "from one point of view there is a truth in catastrophism which is sometimes overlooked by the advocates of continuity and uniformity. Catastrophism has, as its essential feature, the proposition that the known and existing forces of the earth at one time acted with much greater intensity and violence than they do at present, and they carry down the period of this excessive action to the commencement of the present terrestrial order. The Uniformitarians, in effect, deny this proposition, at any rate as regards any period of the earth's history of which we have actual cognisance. If, however, the 'nebular hypothesis' of the origin of the universe be well-founded, as is generally admitted, then, beyond question, the earth is a gradually cooling body, which has at one time been very much hotter than it is at present. There has been a time, therefore, in which the igneous forces of the earth, to which we owe the phenomena of earthquakes and volcanoes, must have been far more intensely active than we can conceive from anything that we can see at the present day. By the same hypothesis the sun is a cooling body, and must at one time have possessed a much higher temperature than it has at present. But increased heat of the sun would seriously alter the existing conditions affecting the evaporation and precipitation of moisture on our earth, and hence the aqueous forces may also have acted at one time more powerfully than they do now. The fundamental principle of catastrophism is therefore not wholly vicious; and we have reason to think that there must have been periods—very remote it is true, and perhaps unrecorded in the history of the earth—in which the known physical forces may have acted with an intensity much greater than direct observation would lead us to imagine."

The truth in these remarks is undeniable; but it strikes us that the difference between these two conflicting views resolves itself after all into the question, When was the "commencement of the present terrestrial order?" Catastrophists admit that the world is *now* governed by law, and that we find merely gradual change, and no evidence of arbitrary intervention, of alternating miracles of destruction and re-creation. But can they fix the date when the "*now*" began? On the other hand, Uniformitarians are quite willing to admit that in the pre-geological ages—which, as Dr. Nicholson intimates, have passed "unrecorded in the history of the earth"—all agencies whose effects we now witness must have been far more powerful than at the present day. But in admitting this we urge that the decline of such forces must have been very gradual, that the difference between *then* and *now* must have been in degree and not in kind, and that consequently, as the author shows, the history of the earth has

always been one of law. Yet Catastrophism overlooks this fundamental fact. It is curious to look back to the assumptions devised by Catastrophists and Brachychronologists to explain away the evidence of the earth's past. Thus Sir David Brewster, whilst fully admitting the traditional longevity of antediluvian man, suggested—to account for the formation of the coal-strata within the six thousand years of what was once the received chronology—that organic life was "once upon a time" briefer and more rapid than it is now, and that whole forests might have sprung up to maturity and passed away in fewer years than they would now require centuries." This introductory section of the present work may be most advantageously studied, even by those who may feel no special interest in the details to follow.

As regards the body of the work, space will not allow us to examine it minutely, chapter by chapter. The facts adduced are, of course, to a great extent the common property of geologists. But even the veriest tyro must, we should think, feel that he is under the guidance not of a compiler stringing together the most interesting—we had almost said the most sensational—matter upon which he has stumbled whilst "reading up" the subject, but of a master in the science who understands the significance of every phenomenon which he records, and knows how to make it reveal its lessons.

The cautions given concerning the possible sources of error in palæontological inquiry are well worthy the attention of the student. Dr. Nicholson reminds us of the frequent absence of fossils in stratified rocks; of the confusion caused by observers in different parts of the world giving different names to one and the same fossil, or, conversely, "lumping" distinct fossils under some common name. These errors are naturally dangerous whenever we argue as to the comparative age of strata from the presence of certain fossil remains. He further shows that the mere fact of a difference of physical position cannot safely be taken into account in determining the true *zoological* affinities of a fossil. Unsafe, also, is the inference that all beds containing similar fossils must be of the same age. The palæontologist may also be led astray by accepting obscure or imperfect physical evidence as to the age and position of some given group of strata.

Concerning the inferences drawn from fossil remains as to the former climate of the locality where they are found, some important cautions are given. The author points out that, as fossils are mostly the remains of marine animals, the evidence obtained bears chiefly upon the temperature of the seas, which, from the existence of currents, hot or cold, does not necessarily prove a corresponding temperature in the adjacent lands. It is, moreover, by no means certain that the "habits and requirements" of any extinct animal were exactly the same as those of its nearest living representatives. Nor is the distribution of species

at the present day dependent upon conditions of climate alone. He does not, however, at all question the conclusion, supported as it is by a vast accumulation of evidence, that the climate of Central Europe in the Eocene period was tropical, whilst that of the Miocene approximated in temperature to Florida or Louisiana.

The view, once prevalent, that fossils are not the remains or casts of animals once living, but *lusus naturæ* formed by some "plastic virtue latent in the earth," or, worse still, as Mr. Gosse most irreverently, in our opinion, insinuates, forged documents to mislead mankind, Dr. Nicholson pronounces contrary to the common sense of scientific men.

The chapter on the "Biological Relations of Fossils," and still more that on the "Succession of Life upon the Globe," bring the author face to face with the great biological question of the day, the origin of species. His views on this subject attract the more interest as he has been to some extent misunderstood, if not misrepresented. Certain of the remaining advocates of the old school of Natural History have invoked him as a kind of successor to Agassiz and an ally of that redoubted dispenser of teleology and magniloquence, Principal J. W. Dawson. But we see no ground on which they can claim Dr. Nicholson as belonging to their fraternity. He decidedly recognises organic evolution. "The palæontologist," he writes, "is so closely confronted with the phenomenon of closely-allied forms of animal life succeeding one another in point of time, that he is compelled to believe that such forms have been developed from some common ancestral type by some process of evolution." If he adds that there "has also been at work some other deeper and higher law on the nature of which it would be at present futile to speculate," he says little more than what every cautious thinker must admit. Natural selection, sexual selection, changing climatic influences, disuse of organs, explain much. Do they explain all? That Dr. Nicholson can rank among the opponents or enemies of Darwin—for there are those who deserve the latter rather than the former appellation—no one who reads the final sentence of this book can admit. "In the successful solution of this problem will lie the greatest triumph that palæontology can hope to attain; and there is reason to think that, thanks to the guiding clue afforded by the genius of the author of the 'Origin of Species,' we are at least on the road to a sure, though it may be a far distant, victory."

The illustrations are numerous, carefully executed, and judiciously selected. To each period is appended a list of the principal works and memoirs which the student may require to consult for more detailed information. There is also an elaborate glossary of technical terms employed, an extensive index, and a "tabular view of the chief divisions of the animal kingdom," in which, however, we regret to perceive that the Mollusca are

made to rank higher than the Annulosa, and that the order Bimana is still retained—two relics of Cuvierianism scarcely on a level with the general standard of the author's teachings. As regards the value of the work there can scarcely exist two opinions. As a text-book of the historical phase of palæontology it will be indispensable to students, whether specially pursuing geology or biology, and without it no man who aspires even to an outline knowledge of Natural Science can deem his library complete.

A New London Flora, or Handbook to the Botanical Localities of the Metropolitan Districts. Compiled from the Latest Authorities, and from Personal Observation, by Eyre Ch. de Crespigny, M.D. London: Hardwicke and Bogue.

The author of this little manual remarks very truly that a new London Flora is much required. The suburban localities for rare or interesting species, such as we knew them in our youth, exist merely in tradition. They have been desecrated by the land-speculator and the "navvy." Woodland, heath, and common now produce little save a crop, more pretentious than interesting, of semi-detached villas. Even where the plague of bricks and mortar has been averted, every wild flower of at all striking appearance, and well nigh every fern save the *Pteris aquilina*, have been long since dug out and carried off to be sold. As the author remarks, "It is a question now-a-days not what there is, but what there is not in the way of rarity." The London district, however, within the average thirty-mile radius taken in this manual, is still rich in species. Of the total 1665 plants which—exclusive of Charads and minor Cryptogams—occur in all Britain—no fewer than 1250 occur in the metropolitan region. Here, however, both author and reader are brought in contact with the vexed question, What is a species? Dr. de Crespigny evidently does not agree with the so-called "splitters," who would elevate every variety to the rank of a species. But for this evil—as it undoubtedly is—the remedy is still to seek. Given a certain group of closely-allied forms, animal or vegetable, one observer may refer the specimens laid before him to ten species, whilst another may conceive that he has proved the existence of twenty, and neither can convict his opponent of error. Does not all this help to undermine the current idea of "species" as an immutable and objective reality?

The author first gives an alphabetical list of species, with their time of flowering and with their localities, save, of course, such as are to be found everywhere. The other main section of the work is devoted to a description of the chief botanical localities, with an enumeration of their productions. We notice

here a mention of Coombe Wood, once so dear both to the botanist and the entomologist, but now shut against both. The author generally mentions such woods and heaths as are closed against scientific exploration—a very useful feature in the manual, which may save many a student from wasting a day. Such restrictions, as far as uncultivated lands are concerned, are, we believe, peculiar to the United Kingdom. Among minor features of the book may be mentioned a list of the authorities consulted, and a list of aggregates, segregates, and synonyms.

To all botanical students residing in the London district this manual will prove a useful guide.

Ferns, British and Foreign: the History, Organography, Classification, and Enumeration of the Species of Garden Ferns, with a Treatise on their Cultivation. By JOHN SMITH, A.L.S. New and Enlarged Edition. London: Hardwicke and Bogue.

No class of plants, probably, is for the present in such general favour as the ferns, and a work like the one before us must consequently find appreciative readers outside the circle of professed botanists. The author is one who has laboured long and zealously in the promotion of scientific horticulture. From 1822 down to 1864 he was curator of the Royal Botanical Gardens at Kew, where he devoted especial attention to this interesting group, and where the number of cultivated species was largely increased by his exertions.

In this Mr. Smith gives a history of the introduction of exotic ferns; an explanation of the terms used in describing them; an account of the principles on which they are classified, with the generic characters and an enumeration of cultivated species. The cultivation of ferns—whether in pots, in the open ground, in ferneries suited for tropical or sub-tropical species, or in Wardian cases—is described at some length, and in a manner which will be decidedly useful to amateurs, now so numerous and so zealous. Here, however, we cannot refrain from quoting with approval a remark of the author's on the present fern-mania:—"However laudable and agreeable fern-growing may be, yet it is to be regretted that it leads to the extinction of some of our rarest native species. Even the more common are becoming scarce in localities within easy reach, great quantities being yearly consigned to the London market." The misfortune is that many fern cultivators allow their pets to die yearly, and purchase a fresh stock on the approach of spring. The extinction of some species in certain localities we have ourselves observed and regretted. Thus we know of several dells in which *Polypodium dryopteris* formerly grew in some abundance, but

where it might now be sought in vain. Perhaps, however, the decrease of our choicer native ferns may be due not so much to the rapacity of cultivators as to the increasing pollution of the atmosphere in many parts of the country. Air fraught with coal-smoke is very uncongenial to many ferns and mosses, and checks their fructification.

In recommending this book to all who are interested in the study and in the cultivation of ferns, we must beg to express our sympathy with the author, who has been deprived of that sense which to all observers of Nature is dearer than life itself.

Narrative of the North Polar Expedition. U.S. Ship Polaris, *Capt. C. F. Hall commanding.* Edited by Rear-Admiral C. H. Davis, U.S.N. Washington: Government Printing-Office.

Polar exploration excites in men of different tastes and pursuits a widely different amount of interest. The biologist looks on almost with indifference, well knowing that a year's labour in the far north would yield fewer results of value than might be met with in a single day in climates more genial. The geologist and mineralogist, though well aware that important portions of the "great stone book" have been, in all probability, deposited around the poles, still fear that accumulations of ice and snow may for ever hide them from human perusal. On the other hand, the geographer, the meteorologist, and the student of terrestrial physics turn their gaze northwards with eager curiosity, and will doubtless never desist from their endeavours to solve the mystery of the poles till their efforts are crowned with success, or till many future adventurers, like Franklin and Hall, have found a glorious death. Nor can the risk and the possible sacrifice be considered to outweigh the prize that remains to be won. Material benefit, indeed, is out of the question. No one now explores the icy regions in the hope of finding an available passage from Europe, or from the eastern shores of North America to the Pacific; nor is the prospect of mineral wealth a temptation, for though valuable ores doubtless lie hid in these regions, the cost and difficulty of bringing them to daylight and transporting them to a market would be scarcely surmountable. But from a speculative point of view the inducements to polar investigations are tempting indeed. Setting aside the extravagant dreams of earlier days, such as the notion of an aperture leading down to beautiful and habitable regions in the interior of our globe, it remains to be decided whether the poles are covered, according to the theories of Adhémar and others, with massive caps of ice miles in thickness; whether they are, as some suppose, covered by open sea, enjoying a milder climate than the

regions immediately around them; or whether they are utterly undistinguished by any physical feature from the arctic wastes already explored. At present we can only infer, with more or less probability, as to what may be the condition of the earth at the extremity of its axis, and with this uncertainty we cannot feel satisfied.

Among the many who have sought to solve the "mystery of the Pole" none can claim a higher place than Capt. Charles Francis Hall, who, if merit could command success, might well have hoped to plant the flag of his country on the most northern part of the globe. He belonged to a class of men for whom we entertain a particular admiration. Like all true specialists, he had devoted his whole being to the solution of one question. For this he had thought, and studied, and prepared himself in every possible way, so that when the crowning opportunity came he might not be found wanting. To this end he sought to stir up public opinion in favour of a polar expedition under national auspices. He lectured in various cities of the Union; he corresponded with official personages, and, in short, left no stone unturned towards the completion of his chosen task. This part of Capt. Hall's life, indeed, reminds us of the journeys of Columbus when he went from court to court soliciting the means to put his project into execution. In pure, self-forgetting devotion to a great idea these two explorers are singularly alike. The American, however, unlike the Genoese, had to deal with a Government which honours and appreciates Science.

The work before us—whilst it recounts the progress of the Expedition, the illness and the lamented death of Capt. Hall, and the heroic struggles of the party after his end—does not detail the scientific results of the Expedition. The various journals and reports in which the observations made by the Expedition lie scattered have not yet been completely digested. Some of the documents, as well as of the specimens collected, appear to have been lost when the *Polaris* had to be abandoned.

Many interesting facts, however, are recorded in these pages. Those who imagine that mosquitoes are an exclusive feature of tropical climates will perhaps be surprised to learn that these pests, along with other two-winged flies, haunt even the far north. Two caterpillars were also found among moss, but of what kind it is not stated. The Mammalia of the region explored consist chiefly of musk-oxen, foxes, seals, lemmings, bears, wolves, and walruses. The Expedition therefore, though it approached the boundaries of animal life, did not actually cross them, as Captain Nares and his followers seem to have done. The fauna and the flora of the two Expeditions, when published, will be welcome as a contribution to our knowledge of the geographical distribution of organic life.

A curious physical fact recorded is the freezing of kerosene oil, which hydrocarbon at $-44°$ F. assumed a consistence like that of

melting wax, and a milk-white colour. No appearance of crystallisation was detected, even under the microscope.

There is one lesson to be drawn from this book which should on no account be overlooked. Copies of the "Narrative" have been forwarded, gratuitously and unsolicited, to universities, to learned societies, and public libraries, as well as to scientific journalists like ourselves, over the whole civilised world. By such acts of munificence the Government of the United States sets an example to its neighbours which we are sorry to see finds but very scanty imitation. Will England ever learn to "Americanise her institutions" in this respect?

Scepticism in Geology, and the Reasons for it. By VERIFIER. London: John Murray.

To this book a preliminary objection may be taken, serious, if not absolutely fatal. The author's ostensible position, as declared in his title and in his preface, is that of the sceptic—using the word of course in its philosophical and only legitimate sense. He selects as his motto the "*Ama nesciri*" of Thomas à Kempis. He calls for more thorough demonstration than geologists have in his opinion been able to furnish hitherto in support of their theories. Far be it from us to question his right to assume such an attitude, provided it be done really and consistently. Like every other science, Geology must be prepared to submit her conclusions to full and impartial criticism. She can claim our assent merely on condition of producing valid evidence. But what is scepticism? It is the dismissal of all preconceived notions bearing upon the question at issue; the refusal to accept without proof any proposition whatever, joined to complete indifference what conclusion may be arrived at provided it be in accordance with the logical interpretation of established facts. To give a familiar illustration, it is the frame of mind which the judge often explicitly enjoins the jurymen in cases which have excited a considerable amount of public feeling. They are told to dismiss all prejudices either for or against the prisoner, and utterly to ignore everything save the evidence which has been presented to them in court. This is true scepticism. But there is a pseudo-scepticism, very common among the opponents of what are called "modern scientific theories," which is combined with the grossest credulity, and which is in fact merely a consequence of the latter feeling. John Nokes doubts very strongly that $a = b$, and considers that in calling for additional evidence he is assuming the position of a sceptic. But what if all his doubts spring merely from a strong preconceived notion—not, let us bear in mind, resting upon any evidence—that a is not equal to b? He is then no sceptic, but a dogmatist in disguise. Such, we are sorry to

find is the real position of "Verifier." His mind, in approaching the question, is not "tabula rasa," but is filled with prepossessions and with an anti-geological—we can scarcely help saying utterly unscientific—dogmatism, which the work before us is an attempt to justify. "The assumptions of modern geology," he tells us, "have filled some minds with alarm." But alarm, or indeed emotion of any kind, is utterly to be avoided by the man who lays claim to the position of a sceptic.

It may seem that we have brought grave charges against the unknown author, but on a careful examination of the book they will appear only too fully justified. He seems to anticipate "rough usage," if noticed at all, possibly from some internal misgivings concerning his "facts" and his "arguments," but, as is common with writers of his school, he appeals to the "candid reader."

Our first proof that he is by no means the disinterested doubter, the modest enquirer he would fain appear, may be found in his remarks on geological time. He tells us "it is necessary to protest against the insatiable demands of geologists for time." He sneers at the observation of Darwin that it cost only 300 millions of years to denude the Weald. He declares there cannot be a more "groundless assumption" than the dictum of Mr. Geikie that "time is power." Can he pretend to misunderstand Mr. Geikie's meaning? "Would it not," he asks, "become the geologists of our time to abandon a position which enforces on their followers a belief almost amounting to a superstition?" Now we defy him to show any inherent or intrinsic improbability in the "untold ages," the "incalculable periods of time" which move his indignation. *A priori*, it is equally likely that the world has existed for six thousand, six million, or six billion years, and those who suppose the latter are not more credulous than those, if any, who still believe the latter. The advocates of Usher's chronology, who dream that "the world was created in autumn, 4004 years before the vulgar Christian era," have certainly no reason to tax geologists with credulity or superstition. Our argument in favour of the long past duration of the earth is drawn from the impossibility of compressing into a shorter time the occurrences of which the "stone book" has preserved the record. His grounds against the "incalculable ages" are never put forward at all, and may therefore, without any want of charity, be set down as a mere prejudice. It is instructive to note how eagerly, in default of any facts to prove the recent origin of the earth, he catches at shadows. He tells us in his Preface—"Darwin, Lyell, and others who proclaim a term of 300 millions of years insufficient for some of the operations of geology, are warned—'So much the worse for geology, since physical conditions render it impossible to allow her more than 10 or 15 millions of years.'" But does he not know that we regard these "physical considerations"—the recently attempted mathematical investigations into the maximum

age of the earth—as scarcely worth the paper upon which they are written? Mathematical demonstration is irresistible when applied to abstractions, but when it seeks to deal with realities it is often vitiated by being based upon false, or at best groundless, premises. The question is quietly and adroitly begged in such expressions as "Let us suppose, "If we only assume," &c., and the public—dazzled and awed by the display of formulæ—never look beneath at the flimsy foundation.

But the lingering belief in the recent origin of our earth is not his only prepossession. Towards the end of the work he declares his belief that the earth has not reached its present condition, but has been finished off at once according to specification, like a contract job—if we may be allowed the comparison—by a First Cause, acting directly and from without! Because the "scientific mind"—which his own is apparently not—repudiates an assumption so degrading, so unworthy of Deity, it is charged with "a stubborn resistance to the belief in a FIRST CAUSE." Can he fail to see that if such be the origin and the earlier history of the earth, the very existence of geology is destroyed? But let us quote some of the passages in which he places his anti-scientific creed in the fullest daylight:—"One vast fallacy would appear to underlie the doctrine of Modern Causes, the supposition that the world we inhabit—so beautiful, so pregnant with every gift which can contribute to man's progress, prosperity, and happiness—has been turned out by its Maker unfinished and imperfect; that it is capable of improvement, at least of development, and is undergoing material change day by day." "Is there any inconsistency in supposing that when a potter moulds a vase out of a lump of clay he should put forth his greatest energy and exert his utmost skill to finish it and turn it out perfect? That the work of the creation of the earth was one of perfection defies all disproof. What need, then, to imagine that it was done by little and little? least of all can we admit a solution of the problem of cosmogony, involving the absurdity that the work was left unfinished and needs constant alteration by means of certain mechanical self-acting operations. We venture to hope that the more geology is studied in an earnest spirit, free from the mirage of attractive but shadowy hypotheses, the more it will be acknowledged that not blind force, however gentle, nor mechanical impulses, however gradual, rendered our planet what we find it. It will eventually be acknowledged that at the time and in the process of fashioning the globe, a power was exerted totally different from the present course of nature." Here, then, the secret is revealed. The author's real difficulties in the way of accepting the truths of geology are his preconceptions of what is an "absurdity," of what is "perfection," and of how, in his opinion, the "First Cause" ought to have worked! With a man who can seriously maintain that the channels of rivers were made for them and not by them, it is impossible to argue. Had he been

a careful and sober-minded inquirer he could not have failed to find the superabundant refutation of what he calls his "arguments" and his "suggestions." He cannot see that "because a puddle in a rainstorm will cut runlets in the soft sand in an hour or two, it must follow that rain or running water will cut glens and valleys, or even sever high mountains, granted millions of years." Minds less filled with prejudices find in this no difficulty. The author ignores the fact that geologists only take up the history of the earth when no longer a nebular or an igneous mass. Consequently when Prof. Huxley speaks of a very remote period when the earth was passing through physical and chemical conditions which it can no more see again than a man can recall his infancy," he does not of necessity contradict Lyell when the latter declares that "the forces now operating upon earth are the same in kind and in degree as those which in the remotest times produced geological (but not pre-geological) changes."

"Verifier" asks—"If Nature were still carrying on operations by which the globe was made and fashioned, only on a diminished scale, are we not in the first place entitled to expect to catch her in the act of producing some of those *elementary* substances which enter into the composition of the earth's crust; not of depositing but of creating the metals and simple minerals, gold, silver, tin, quicksilver, iron, the diamond, emerald, &c.? In no instance has any such discovery been made." This is obviously an adaptation of the well-known anti-Darwinian argument, that no one has ever seen an animal or plant in the act of evolution. This objection is obviously of very little value, but as applied by "Verifier" it is even more unreasonable than in its original use. The transformations of organic beings might naturally be expected to take place on the surface of the earth, and if of a nature to attract attention might conceivably be noticed by man. But if *elementary* substances are being now created there is a very strong probability that the operation would take place underground, where no human eye could possibly witness. Again, is it not probable that all the materials of which our reputed elements consist have been used up in the production of such elements? It will be quite time enough to ask why we never witness the creation of gold when our chemists have detected a something out of which it could be created. Further still, supposing that tin or iron were in some place being actually created and not deposited, how could we assure ourselves that such was the fact? Suppose "Verifier" is standing by the side of a rock and sees a deposit of any metal forming upon its surface, and that with sufficient speed to be noticed, is he aware of the precautions necessary to prove that this is not a mere case of deposition? He tells us that "the sand and mud washed down into the Mediterranean by the Rhone in the days of Hannibal remain to this day incoherent sand and mud." How does he know this? and, painful as the question may sound to him, what is it to the purpose if

so short a time of exposure to the action of the sea should produce no decided action? We must therefore utterly repudiate his "obvious conclusion" that the conditions under which these substances were formed exist no longer.

"Verifier" next takes his stand upon metamorphism, by which he says, "sandstone may have been converted into quartz-rock, or even into granite." He ought to be aware that sandstone or quartz alone can never form granite, as two other constituents are requisite. To some, he tells us, "hot water charged with chemical carbonates seems to have been the agent" of metamorphism. Why some carbonates should be designated as "chemical" we are not sufficiently versed in pseudo-scepticism to comprehend. He asserts that "it has been impossible to realise metamorphism, or even to imitate it in the laboratory," and imagines he has reached "one unmistakable conclusion, viz., that metamorphism is a thing of the past, its processes not now discoverable, and it must therefore be dismissed from the category of 'Causes now in action.'"

If, however, the author had taken the trouble to make himself acquainted with the results of modern scientific research, he might have become a wiser and a less confident man. The researches of Messrs. Daubrée, Sterry Hunt, and others have decidedly proved that metamorphism can be imitated in the laboratory, and that neither a large quantity of water nor a very high temperature is requisite. We would recommend him to read the "Annales des Mines" (Ser. 3, vi., 78; Ser. 5, xii., 289; Ser. 6, vi., 78), "Memoires Academie des Sciences" (xvii., 1860), "Bulletin de la Soc. Geol. de France" (Ser. 2, iii., 547), "Ann. de Chimie" (xxiv., 258), "Quart. Journal Geol. Soc. of London" (xv., 488). His assertion that metamorphism is not still going on is utterly unproven, and it is, to say the least, contrary to all probability. There are certainly metamorphitic rocks which have been produced at comparatively recent dates. From the very nature of the case we cannot expect to "assist" —in the French sense of the word—at the process. Why metamorph products should be more common among the older than the more recent formations must be self-evident to every geologist.

Turning to another part of the book, we find the assertion that earthquakes are most common in plains and low grounds. This is exactly the reverse of the truth: the great earthquake regions—the western coast of South America, the West Indies, New Zealand, Iceland, Japan, the Malay Islands (excluding Borneo, which is more level), Calabria, the Iberian Peninsula, and the Greek Islands—are all mountainous. On the other hand, the vast plains of Eastern South America, of Central and Eastern Europe, and Northern Asia, are almost free from earthquake action.

There can surely be no occasion for us to wade further through

a book in which the plainest facts are denied or misinterpreted, and in which the all-important evidence of palæontology on the former elevations and subsidences of lands, and on their alternating connections and disconnections, is left out of view. A real "Scepticism in Geology" would be welcome, and would do the world good service, but it must produce very different evidence and be written in a totally different spirit from the book before us. "Verifier" has been nine centuries too late.

A Treatise on the Law Relating to the Pollution and Obstruction of Water-courses, together with a Brief Summary of the Various Sources of Rivers Pollution. By CLEMENT HIGGINS, M.A., F.C.S., Barrister-at-Law. London: Stevens and Haynes.

IT is only under very exceptional circumstances that we can, with either benefit to our readers or credit to ourselves, presume to give an opinion concerning the merits or the defects of a work on any branch of law. Even in a case like the present, where we may lay claim to some acquaintance with, and at any rate a deep interest in, the subject-matter of the laws expounded, we can only deal with what may be called collateral issues. We may be able to discuss the question as to what constitutes "pollution" in a river. We may show the impracticability of certain standards that have been laid down, and point out others more feasible and rational. But the interpretation of statutes we must dismiss as a matter not within our competence.

The treatise before us is divided into two parts. The first of these is devoted to an exposition of the "Rivers' Pollution Prevention Act" of 1876. The author examines what constitutes an offence under the Act as to solid matters, sewage pollution, manufacturing refuse, and mining pollutions, and then treats of the institution of proceedings, and of facilities for conveying the refuse of factories into sewers. It is in this part of the work only that certain chemical questions arise. What constitutes "pollution" is the first difficulty. The author remarks that "The successful working of the Act will much depend upon the meaning placed upon the word "polluting" as therein used by those with whom its interpretation rests. It may be comparatively easy to determine whether any particular manufacturing, mining, or other refuse is poisonous or noxious, but it is certainly far more different, on account of the different meanings placed upon the word, to say whether it is or is not polluting. No attempt has been made by the framers of the Act to define the word, but they are content to say merely that the term "polluting" shall not include innocuous discolouration."

This exception seems to us an element of danger. No one

can contend that the colouring matters of logwood, fustic, peachwood, &c., if present in a river are likely to be injurious to man, beast, or plant. But who knows what accompanying and really noxious pollutions they may hide? On this account we think that transparency and the absence of colouration are characteristics to be desired, especially as they may be attained by known methods.

The author continues:—"Neither do the decisions of the judges either in courts of law or equity furnish a definition of the word such as to meet the requirements of the Act." As the only way of solving the difficulty he gives a summary of the expressed opinion of scientific men. Under this head we find once more the recommendations of the Rivers' Pollution Commissioners, on which surely nothing further need be said, especially as they have not been deemed fit for legislative adoption. Mr. Higgins gives along with them some—though by no means all—of the objections raised against them. He then quotes with marked approval the two recommendations given by Mr. Crookes, F.R.S., in his evidence before the Committee of the House of Lords. These are simply that the river itself should be the standard of purity, and that no liquid should be permitted to fall into a river if such liquid contains a greater percentage of impurity than the river itself. These suggestions, Mr. Higgins thinks, may be safely followed by county court judges in their decisions under the Act.

As regards solid matter thrown into rivers another difficulty arises. "No offence against the Act is committed unless the solid refuse, rubbish, cinders, or other waste or putrid solid matter finding its way into the river results either in *interfering with its due flow* or in *polluting its waters*." Here, we fear, the Act is scarcely stringent enough. The gradual silting up of rivers from natural causes is a source of danger which requires to be jealously watched and contended against, as the records of the past winter will show but too plainly. Surely, then, it is intolerable that the evil should be intensified by the sloth or the greed of those who would make the rivers general depositories of filth and refuse of all kinds. On this subject the Rivers Pollution Commissioners in 1868 spoke very judiciously. They recommended that not merely "the casting of solid matter of whatever kind into rivers and running water" should be prohibited, but also the placing of solid refuse in such positions on the banks of rivers as to render it liable to be washed away by floods." They also suggested that any Act passed for this purpose should take effect immediately.

In treating of sewage and of liquid manufacturing refuse, Mr. Higgins, though himself a chemist, and far better qualified to speak on the subject than many who have come forward as the instructors of the public, does not enter into the comparative value or efficiency of the various methods of dealing with polluted waters.

He even reminds manufacturers, municipal and sanitary authorities, &c., that "it is not the business of the Courts to give instructions in sewage purification.

The second part of the work is devoted to "Riparian Rights and their Protection." The author treats of such rights arising *ex jure naturæ*, or by way of easements or custom, of their protection by injunction or by an action for damages. The appendix contains statutory provisions relative to water-courses and the vesting of sewers, There are also tables of statutes and of cases.

As far as we are capable of judging this work is a valuable contribution to the literature of the sewage question, the law of which, as well as its chemistry and its engineering, requires to be brought clearly before the public.

The New Formula for Mean Velocity of Discharge of Rivers and Canals. By W. R. Kutter. Translated from articles in the "Cultur-Ingénieur," by Louis D'A. Jackson, A.I.C.E. London and New York: E. and F. N. Spon.

The author tells us in his preface that "while the lead in engineering progress generally, both theoretical and practical, seems to have been almost entirely taken by the English-speaking races, and whilst improved construction, perfected appliances, and higher economy have progressed in the last thirty years at a speed perhaps greater than has ever been previously known, yet in the hydraulic branches of engineering no similar claim can be very satisfactorily made out for our own country. This seems at variance with our present requirements.

"We have in India a vast empire existing in a state of mutual dependence with England, whose enormous wealth is dependent on its population, whose population is dependent upon agriculture, and whose agriculture depends chiefly upon irrigation; where water is like silver, and the science of its judicious application and control is like gold. We have in semi-tropical regions large colonies which suffer from devastating floods alternating with drought." Thus far we are with the author; the evils which he has enumerated might doubtless be obviated by a proper system of reservoirs to catch the rains when they fall; of by-washes to carry the surplus part through towns and cultivated lands without occasioning devastation, and of conduits for the judicious distribution of the stored-up water in the dry season. To do these things is the work of the hydraulic engineer, and if he is to do them well he must not be required to work upon erroneous data. Two incidents in our modern domestic history —the Holmfirth and the Sheffield floods—confirm the author's view that hydraulic science—at least in its practical applications —has not progressed amongst us as might be desired. Is it

creditable to us that work done in præ-historical times of Ceylonese engineers should be better than some completed in England in the nineteenth century?

But Mr. Jackson goes on to say:—"At home the catchment areas of our rivers, in fact the country generally, is in a polluted state, the drainage both from farm lands and townships being still either badly regulated or under no general control. In spite of the increasing exceptions the water supply of most of our towns is so contaminated as to conduce, among other evils, to a fearful amount of intemperance, and the sewage, the natural regenerator of soils and crops, is generally allowed to mingle with noxious refuse, or to be so ill-regulated as regards dilution and application to land, that it not only ceases to be useful, but becomes a source of perpetual pollution." Here, we fear, our author is losing his way. We are not aware that intemperance is less rampant in Liverpool, Manchester, Glasgow, Leeds, Halifax, Huddersfield, Sheffield, and other towns supplied with uncontaminated waters from the mountains, than in such as draw their water from questionable sources. The selection of water and the disposal of sewage are, in our humble opinion, chemical not hydraulic questions. Further, defective as may be our water supply and our systems of drainage, we have still the satisfaction of knowing that in such points we are certainly not in arrear of our continental neighbours. The sanitary shortcomings to which we must plead guilty are due more to an over-tender regard for "vested interests" than to defective knowledge.

The text of the work before us is devoted to the flow of water in open channels generally, and secondly to the flow in open channels in earth. Then follow tables of the coefficients of mean velocity of discharge, of discharges and mean velocities per second, and a supplementary table of percentages for certain seconds. The work is a most valuable addition to English engineering literature, and we can only regret that it did not appear at an earlier date.

A Treatise on the Trisection of an Angle of Thirty Degrees and of any other Plane Angle. By Bernard Tindal Bosanquet. London: Effingham Wilson.

The author of this little pamphlet tells us that "with regard to his theory for the trisection of an angle of thirty degrees, he feels bound to admit that he has grave doubts as to its truth, having failed altogether in finding a geometrical solution." A fair examination of the question would be impossible without the aid of the author's diagrams.

SCIENTIFIC NOTES.

The Meeting of the British Association for the Advancement of Science for 1877 was held at Plymouth, under the Presidency of Prof. Allen Thomson, M.D., LL.D., F.R.S. The attendance at the meeting was below the average, only 1217 tickets being issued. Several papers of great scientific interest were, however, read: we may mention in particular Sir William Thomson's paper on "The Possibility of Life on a Meteoric Stone falling on the Earth," and Mr. Preece's paper on "The Telephone," both read in Section A. Great amusement was caused by Sir William Thomson saying that, though the outside shell of a meteoric stone might be incandescent from the friction caused by its flight through the terrestrial atmosphere, yet within a crevice of that stone might be concealed a Colorado beetle, which, falling on the earth, might become the father of a large and prosperous family. The lecture to working men was delivered by Mr. Preece, the subject being "Telegraphy." At the conclusion of the lecture the telephone was connected with the Plymouth and Exeter Post-offices, and ordinary telegraphic operations were suspended, in order that the respective places might be communicated with through the telephone. The experiments were conducted by the President, Mr. Preece, and Sir William Thomson, the latter gentleman remarking that the sounds came more perfectly than he had ever heard through a speaking tube. At the final meeting of the Mechanical Science Section the telephone was also exhibited and described by the inventor, Mr. Graham Bell, who arrived from the United States during the meeting of the Association. Mr. Bell, after observing that many years ago his attention was directed to the vibrations occasioned in the air by vocal utterances, gave a history of the experimental researches which he had conducted with the view of discovering means by which the sound of the human voice could be successfully conveyed to whatever place was desired, and which had led to the invention of the telephone. He had four different kinds of instruments, and was uncertain which was the best; but experiments were being conducted at the present time for the purpose of making improvements and developments. The invention was therefore still in embryo. Mr. Bell then announced that he had brought with him his telephonic organ, which resembled a harmonium or parlour organ. The reeds were connected with a battery, and in front of each reed there was a little screw with a platinum point. When the instrument was blown the reeds vibrated against the screws, which were connected with a telegraph wire, and on contact being made the music was conveyed. The organ was at the Guildhall, and was connected by a telegraph wire with both the room and the Post-office, the battery being at the latter place. Mr. Preece then attached the telegraph wire to a telephone, and inquired if his assistant, Mr. Harris, was at the Post-office. The immediate reply was "Yes, sir." Mr. Preece then said, "Will you put the wire through to the Guildhall, and ask the player there to give us a tune?" "Right; I will do it at once," was the answer. After a few moments the strains of "God Save the Queen" were distinctly heard by the audience. Mr. Bell explained that the sounds had been produced by a voltaic battery, and the music should next be without the aid of a battery. The sounds would be much fainter, but might be audible to those who were near the platform. The "National Anthem" was again played, but less distinctly than before. At the request of the professor Mr. Harris sang a song through the telephone from the Post-office, and "Auld Lang Syne" was heard with great clearness.

At the end of the Plymouth Meeting of the British Association, Sir William Thomson alluded to the great loss the Association has sustained in the death

of Mr. Gassiot, F.R.S. He alluded to the interesting communications of experiments made by Mr. Gassiot in Section A at Brighton, and to the enormous harvest of results that had come into it as the result of his munificence. It was through Mr. Gassiot's foundation of £500 a year that the maintenance of the Kew Observatory was ensured. That was a foundation in perpetuity, and the results already derived from it were of such obvious importance in connection with that Section that it was right they should consider them now the founder was no more. Mr. Gassiot's life had been one of usefulness—usefulness in business—among his friends; and, above all, in the wider scientific sphere in which the later years of his life had been employed. That he should have lived so long—over 80 years—and won so much good feeling, respect, and admiration, was enough to make him, whether in life or in death, to be equally envied.

A Special Meeting of the California Academy of Sciences was held on August 30th, for the purpose of extending a formal welcome to Sir Joseph Hooker, C.B., Dr. Asa Gray, and Prof. F. V. Hayden. The chair was taken by Prof. Davidson, the President of the Society. In returning thanks for the cordial welcome given, Sir Joseph Hooker said:—"The President has asked me to say a few words with respect to the Academy. In England we know well enough what it is to wait for results; but the destinies of Science on this coast are great, and a time will come that will show great results, and that will come with immense force, and for these two reasons:—There is here a most intelligent and a most active and progressive population, and, in the second place, there is here one of the most remarkable assemblages of natural objects and physical phenomena that any part of the world possesses. In speaking thus I include the whole coast north and south of California. There is no section of the earth in which so many singular phenomena can be observed as in this. Without seeking to give advice I may point out what has been the element of success in the greatest Academy of England, the Royal Society. It began with very few men, and for the best part of two centuries it was supported by what he might, without disrespect to his ancestors in science, call elderly people. It was by the elderly men who loved Science, holding together congenially year after year, and almost century after century, that the young men of the Society were drawn to it, and it is but lately that young men in any numbers have come into the Society. For success there are three principal elements—the holding together of the elderly members, of those who have had experience of this life in other matters than Science, and who bring that experience together, with methodised common sense, of which Science consists, to bear upon the objects of the Society itself. In the second place, there is the important work of the Secretary, together with that of the Publication Committee, which should carefully pass judgment upon the communications to be given to the world. The supervision of the papers of a Society by several members is perhaps the most important scientific work that any Society can perform. Thirdly, there is the necessity of looking well after the funds, and managing them with economy and prudence." Prof. Hayden, in responding to the welcome, indicated the features of the geological survey in progress under his direction, and said he has long desired to make some comparison between the Sierra Nevada Mountains and the Rocky Mountains. It had always been his belief, although the belief had been corrected by his studies of the Eastern Slope, that there is a general geographical as well as geological unity in all the different ranges of mountains that compose our country. Other geologists have endeavoured to give to the Sierra the name of the Cordilleras, as a generic term, extending it to the Andes and to the Eastern Range, the Rocky Mountains. Other geologists have sought to make the Rocky Mountains the generic name, including in that range all the rest, and making the Sierra Nevada a branch. He was now inclined to think there is difference enough in the two ranges to regard them as separate, and perhaps almost independent ranges. One object of his visit was to examine the Yosemite Valley, and study the phenomena of its formation, and this he had been enabled to do. At some other time he hoped to be in a position to study the geology of the coast carefully.

Geology, Palæontology, &c.—Dr. F. V. Hayden has issued further valuable Reports on the progress of the U.S. Geological Survey of the Territories. The "Preliminary Report of the Survey of Montana and Portions of Adjacent Territories, being a Fifth Annual Report of Progress," contains, among its more important features, Dr. Peale's report on the minerals, rocks, and thermal springs of the region; Prof. Thomas's remarks on its agricultural resources; palæontological notices, by Messrs. Lesquereux and Cope; and an account of the present zoology and botany of the territory, by Messrs. Leidy, Horn, Uhler, and Porter. Turning first to the last-mentioned subject, we find it recorded by Dr. Horn that, in the Coleoptera at least, the higher the elevation and the colder the climate the more deeply sculptured is the species. "Species appear to be distributed on lines of country presenting as nearly as possible similar meteorological conditions. Thus many Oregon forms extend southward into California, gradually seeking a higher mountain habitat as the region becomes warmer." The catalogue of Coleoptera will prove suggestive to all who are interested in zoological geography. As regards the classification adopted we cannot but protest against the group "Scarabæidæ," made to include all the Lamellicornes except the Lucanidæ, and forming thus a division of a very different rank from the "Scarabæidæ" of MacLeay. The well-known potato-beetle, *Doryphora decemlineata*, is indeed mentioned in the catalogue, but the curious fact is noticed that its progress is not westwards. The Hemiptera of the territory are not merely catalogued, but described systematically. The Orthoptera, which here include many species formidable to the agriculturist, are also described at length. The palæontological report is exceedingly interesting. Among the conclusions drawn from the fossil botany of the region is that the "Tertiary flora of North America is by its types intimately related to the Cretaceous flora of the same region. All the essential types of our present arborescent flora are already marked in the Cretaceous of our continent, and become more distinct and more numerous in the Tertiary; therefore the origin of our present flora is, like its *facies*, truly North American. The relation of the North American Tertiary flora with that of the same formation of Europe is marked out for North American types, but does not exist at all for those which are not represented in the living flora of this continent; therefore the European Tertiary flora partly originates from North American types, either directly from this continent or from the Arctic regions. The species of plants common to the Cretaceous and Tertiary formations of the Arctic regions and of our continent indicate in the mean temperature influencing geographical distribution of vegetation a difference in +, equal to about 5° of latitude for the Tertiary and Cretaceous epochs." The paper on the geology and palæontology of the Cretaceous strata of Kansas opens with a forcibly-written general sketch of the ancient life. In the remains of huge reptiles the district is exceedingly rich. "If the explorer searches the bottom of the rain-washes and ravines he will doubtless come upon the fragment of a tooth or jaw, and will generally find a line of such pieces leading to an elevated position on the bank or bluff where lies the skeleton of some monster of the ancient sea. He may find the vertebral column running far into the limestone that locks him in his last prison, or a paddle extended on the slope as though entreating aid; or a pair of jaws lined with horrid teeth, which grin despair on enemies they are helpless to resist." Twenty-four species of fossil reptiles have been found in Kansas, varying from 10 to 80 feet in length, and representing six orders. Two only were terrestrial, two were winged, and the remainder were oceanic. The rulers of the waters of ancient America were the Pythonomorphæ—creatures much resembling monstrous serpents, but furnished with two pairs of paddles like the flippers of a whale. One of the species, *Liodon dyspelor*, appears to have reached the length of 75 feet. Among the flying saurians the most formidable was *Ornithocheirus umbrosus*, which measured nearly 25 feet across its wings. Among the illustrations will be found a map of the great national Yellowstone Park, and plans, and views of the geysers which appear to surpass those of Iceland.

Under the title of "Preliminary Report of the United States Geological Survey of Wyoming and Portions of Contiguous Territories," we find a general review of the geology of the country from Omaha to Salt Lake Valley. Now that catastrophism is again attempting to raise its head, and now that outsiders are hugging themselves with the notion that the "guns of geology are being turned upon biology," one passage in this review becomes doubly important. We may parenthetically point out the inconsistency of the "rump" of the Cuvierian school, who one moment declare that Evolution must be a myth because certain species still surviving can be traced back to the earlier geological epochs, but the next pronounce it baseless because from time to time every living being has been swept away by some violent and general convulsion! The writer of the work before us remarks:—"We have already obliterated the chasm between the Permian and the Carboniferous era, and shown that there is a well-marked inosculation of organic forms, those of supposed permian affinities passing down into well-known carboniferous strata, and admitted carboniferous types passing up into the permian. We believe that the careful study of these transition-beds is destined to obliterate the chasm between the Cretaceous and Tertiary periods, and that there is a passing down into the Cretaceous period of tertiary forms, and an extending upwards into the Tertiary of those of cretaceous affinities. . . . If there is a strict uniformity in all the operations of Nature when taken in the aggregate, as I believe there is, then this is simply in accordance with the law of progress, which, in the case of the physical changes wrought out in the geological history of the world, has operated so slowly that infinite ages have been required to produce any perceptible change. The position I have taken in all my studies in the West is that all evidences of sudden and paroxysmal movements have been local, and are to be investigated as such, and have had no influence on the great extended movements which I have regarded as general, uniform, and slow, and the results of which have given to the West its present configuration." In a chapter on the fossil plants with reference to our present civilisation, the difference of origin between coal and petroleum is thus explained:—"Petroleum is, like coal, the result of slow maceration of plants; with this difference, that in the formations of the coal the plants which entered into the composition of the matter were woody or fibrous, and the woody tissue cannot be destroyed by the process of slow combustion any more than it is in burnt charcoal. The plants which concurred to the formation of petroleum were sea-weeds. These have no fibre, no wood in their tissue, which is merely cellular, and in their decomposition all trace of this tissue has been destroyed and pure bitumen preserved, either by impregnation of shale or sandstone or by accumulation in subterranean cavities." It is to be regretted that matter so interesting should be conveyed in a style so cumbrous and feeble. Near the junction of the White and Green Rivers, on the borders of Colorado and Utah, is an immense tertiary deposit, consisting of petroleum shales abounding in the impressions of leaves and of various insects:—"Between sixty and seventy species of insects were brought home, representing nearly all the different orders: about two-thirds of the species were flies, some of them the perfect insect, others the maggot-like larvæ; but in no instance did the imago and larva of the same insect occur. The greater part of the beetles were very small. There were three or four kinds of Homoptera, ants of two different genera, and a poorly preserved moth. Perhaps a minute *Thrips*, belonging to a group which has never been found fossil in any part of the world, is of the greatest interest. It is astonishing that an insect so delicate and so insignificant in size can be so perfectly preserved in these stones: the wings remain uninjured, and every hair of their microscopic fringe may be counted." Here, as in other parts of the world, we notice among the fossil Mammalia forms which combine the characteristics of what are now distinct species, or even genera, and are thus important evidence in favour of the doctrine of Evolution. From the Bridger Rocks Prof. Leidy reports the former existence of an animal possessing an affinity to the hyæna and the panther. It was larger than the couguar (the so-called "panther" of the United States), and was a predaceous animal of great strength, and doubtless of equal ferocity.

"It has been named *patriofelis ulta*, which signifies the ancestral cat that hath revenged itself." How it has avenged itself, or upon whom or wherefore, we do not learn. Still more remarkable is the *Hyænodon*, three species of which have been discovered in the Lower Miocene tertiaries of White River. In their anatomical character they approach at once the wolf, the tiger, the hyæna, the weasel, the racoon, and the opossum. *Hyænodon horridus*, three imperfect skulls of which have been found, was doubtless the most formidable carnivorous land animal of its time. Another of these missing links or intermediate forms is Dr. Leidy's "ruminant hog." Like the tame swine it was provided with incisors and canines, but its molars were constructed upon the pattern of living ruminants. They were four-toed, and unprovided with horns. The former development of the camel and of the horse tribe in America is most remarkable. The only camel-like animals at present existing in the western hemisphere are the llama and its congeners, in the mountainous parts of South America; yet in the valleys of the Niobrara and the Loup Fork are found the fossil remains of a number of species of extinct camels, one of which was of the size of the Arabian camel, a second about two-thirds as large, and a third smaller. In like manner no horses existed in America prior to its discovery by Columbus; yet Dr. Leidy reports twenty-seven species of *Equidæ* which lived on this continent prior to the advent of man, "about three times as many as are now found living throughout the world." Among the Pachydermata were a true hog as large as the African hippopotamus, five species of rhinoceros, a mastodon, and a large elephant not as yet discovered in any other part of the world. The author is inclined to infer, from the totality of the phenomena observed, that the Western continent is the original home of animal life, and instead of being the "New World" is older than the land in the Eastern hemisphere. The fossils found are remarkable for their beauty and perfection, and cannot have been transported from a distance or exposed to the action of turbulent waters, as they "seldom show any signs of having been water-worn, and the sharp points and angles are as perfect as in life." Turning from fossilised to living beings, we find an elaborate and interesting account of the *Caloptenus spretus*, or hateful grasshopper—a scourge in its native haunts, at least, more formidable even than the Colorado beetle, but fortunately less able to accommodate itself to differences of climate, elevation, and soil. Did space allow we might go on almost indefinitely multiplying interesting extracts from this volume. One defect in the getting up of the work we must beg to point out:—In many scientific books the heading to each page is a key to the subject-matter below. In others the heading of the right-hand page alone serves as a running table of contents, whilst that of the left-hand page merely repeats in brief the title of the book; but in the volume before us each page is merely headed "Geological Survey of the Territories," which makes it difficult to find any particular passage of which the reader is in quest.

No. 3 of vol. iii. of the "Bulletin of the United States Geological and Geographical Survey of the Territories" contains articles on the comparative vocabulary of the Utah dialects; on the method of making stone weapons; on a peculiar type of eruptive mountains in Colorado; notes on the geology of the region of the Judith river, Montana, and on vertebrate fossils found near the Missouri: with a series of palæontological papers referring chiefly to Unionidæ, Physidæ, and other Mollusca. Among the fossils from the Judith river are a number of the bones of a large Deinosaurian reptile which show not merely Avian but also Mammalian affinities, such as the great coronoid process of the dentary bone, which is a feature unknown among birds and reptiles.

The "Miscellaneous Publications" include "Lists of Elevations, principally in that portion of the United States West of the Mississippi River." The number of summits in the Rocky Mountains ranging from 12,000 to 14,000 feet above the sea-level is remarkable, but greater heights are rarely attained. The loftiest peaks are Mount St. Elias, in Alaska, variously stated at from 16,938 to 19,500 feet above the sea-level, and Mount Cook, also in Alaska, 16,000 feet.

The Director of the Geological Survey of Canada, A. R. C. Selwyn, F.R.S., F.G.S., has issued the "Report of Progress for 1875—1876." The Dominion, like its southern neighbour, the American Union, is sparing no pains in ascertaining the nature and the magnitude of its natural resources. For this purpose exploring expeditions are being sent to report on the mineral wealth of the back territories, their agricultural capabilities, their climate, and the means they offer for communication. Nor are these investigations limited to the economic features of the country. Its geological characteristics, its fauna and flora, both living and extinct, are carefully scrutinised. That this conduct is true wisdom, even from a practical point of view, stands in no need of demonstration. The general impression which must be left on the mind of the reader is that the value of the western, and even the northern, parts of the Dominion is far greater than we in England commonly imagine. Thus even in Fort Simpson—latitude 62° N.—barley always ripens between the 12th and 20th of August, and wheat succeeds in four seasons out of five, which is more than can be said of some localities in England. Vancouver Island and the Queen Charlotte group resemble in their climate the western shores of Ireland, but are warmer and less troubled with summer rains. A current analogous to the Gulf Stream, the "Kuro Siwo," is the cause of a temperature abnormally high for the latitude. The forest growth is magnificent, the soil fruitful, the deeper strata rich in gold, silver, coal, and iron. The coast-line of 400 miles in extent is indented with inlets—here called canals—resembling the fiords of Norway, and affording everywhere safe anchorage for ships. In short, it would be difficult to imagine a region better fitted by Nature to be the home of a great industrial and commercial people. The botanist, Mr. Macoum, speaks with enthusiasm of the beauty and luxuriance of the vegetation, of which he gives lists. This volume contains an obituary notice of Sir W. E. Logan, the first Director of the Geological Survey of Canada, to whom, indeed, the success of this undertaking is very largely owing. It is scarcely needful to remark that this Report, like its predecessors, constitutes an invaluable storehouse of facts for all who are interested either in the geology, the palæontology, or the botany of the Dominion. Its utility is enhanced by the accompanying maps and views of the scenery taken from photographs.

From Section XI. of Dr. Ottokar Feistmantel's "Memoirs of the Geological Survey of India—Palæontologia Indica, being Figures and Descriptions of the Organic Remains procured during the Progress of the Geological Survey of India," which treats of the Jurassic (oolitic) flora of Kach, we learn that the flora of Kach is very poor when compared with the other fossil floras of India, especially in the number of species. In age it probably approximates to the Lower Oolite of Yorkshire, as seen at Scarborough, with which it has eight forms identical. With the oolitic floras of France and Italy that of Kach has no species in common.

Microscopy.—"Double-Staining Tissues with Indigo-Carmine and Carmine," by F. Merbel ("American Journal of the Medical Sciences," for January, 1877). The dye consists of two fluids made separately, but mixed before use. One, a boracic solution of carmine (carmine, ½ drachm; borax, 2 drachms; distilled water, 4 ounces). The other, a similar solution of indigo-carmine (indigo-carmine, 2 drachms; borax, 2 drachms; distilled water, 4 ounces). Indigo-carmine is the trade name for sulphindigotate of potassium, and is the same dye used by Chrzonszczewsky in his researches upon the commencement of the portal duct. The specimens to be stained, if hardened in chromic acid, must be deprived of it by washing, and then be immersed in the mixed dyes for a quarter of an hour. After that they must be transferred to a saturated solution of oxalic acid, both to "set" the blue colour as well as to lighten the general tint, and then, having been washed in distilled water, mounted in Canada balsam in the usual way. The author does not consider the process as yet perfected, but hoped that, by finding some other reagent than oxalic acid, that would possess its good without its deleterious properties, a more uniform and certain result might in all cases be ensured.

The Rev. A. Renard, of Louvain, has contributed to the "Transactions of the Royal Microscopical Society" a valuable treatise on the "Composition and Microscopical Structure of the Belgian Whetstones." The rocks examined were those in the neighbourhood of Viel-Salm, in the province of Liege, Belgium. The stones are in shape parallelopipeds, composed of a stratum more or less deeply coloured yellow, and of a stratum coloured blue-violet, and constitute the well-known "razor-hone." The hones of Viel-Salm are found amongst the rocks which Dumont calls Salmien, but seem to resemble very much the English Cambrian, and to be the equivalent of the schist of Tremadoc. The blue and yellow layers, although apparently separated by a distinct straight line, are so intimately united that the workman has nothing else to do than to square the fragments. The minute structure of the rock is very remarkable. A fibrous pliation oblique to the stratification is prolonged from the slate into the whetstone; and if the slate be broken the lamina comes off, passing across the whetstone and the fissure is prolonged with a regularity and constancy of angle which shows evidently that the two rocks have a common cleavage. The author is of opinion that the whetstones are contemporary layers with the incasing slates, and have been subjected to the same mechanical actions as all of the phylladic rocks of this group. The mineral composition and the microscopical and optical structure have been carefully studied by the Abbé Renard. The matter is highly interesting, but does not admit of a useful abstract being made, and occupies too much space to quote in full. The paper affords another instance of the great value of the microscope, and especially the service rendered by the polariscope to the investigation of substances hitherto regarded as almost structureless.

Specimens of fossil Polyzoa are frequently obtained in which the least interesting side is the one exposed to view. Mr. John Young, of the Hunterian Museum University, of Glasgow, has communicated to "Science Gossip" (July, 1877, p. 158) a process by which the specimens may be detached from the original matrix, and mounted so as to show the side hitherto out of view. The specimens suitable for the process are those imbedded in shales that yield readily to the disintegrating influence of the weather. Very little can be done with specimens imbedded in hard calcareous shells or limestone. After selecting the specimens of Polyzoa it is best to let them be well dried at a fire or in the sun for a few days, to secure the adhesion of the asphalte used in mounting. When a specimen is to be operated upon let it be heated before the fire, and a layer of asphalte melted over it with a piece of iron heated nearly to redness. The asphalte used is the common sort, free from sand, employed in street paving. While hot, press a piece of tough brown paper down over the surface evenly with the fingers. The paper strengthens the asphalte very much, and afterwards, when the specimen is mounted, the paper adheres to the tablet on which it is fastened better than when asphalte is used alone. When large fronds of Polyzoa have to be lifted from the shale, then a second layer of asphalte and brown paper is employed: this forms a firm thin cake, which in large specimens is less likely to break across. The next operation, after fixing the asphalte to the fronds of the Polyzoa, is to place the specimens in water, and let them lie until the shale softens. In some cases the Polyzoa parts from the shale in a few minutes; in others an hour or two, or even a day, may be required. The process may be hastened by placing the specimens in a saucer filled with water, and, as the shale is softened, keep picking it away with a thin sharp knife until the fronds of Polyzoa appear; then with a worn nail or tooth-brush mash the surface of the specimens until they are quite clean and the cell-pores well exposed. If the fronds have been well fixed to the asphalte, the greatest freedom may be used in mashing the specimens without fear of their removal by the brush. For cleaning small specimens of Polyzoa intended for microscopical examination, the following method may be employed:—After having picked the specimens out from amongst the weathered limestone shales, where they often have a thin layer of clay adhering to them, take a glass slide and cover it with a layer of thin gum; then with the forceps lift all the fragments of Polyzoa to be cleaned, and place them on the slide with the poriferous face uppermost, afterwards

allowing the slide to dry slowly for a day or two. When the gum is quite hard, place the slide in a saucer of water, and brush the specimens gently and quickly with a nail- or tooth-brush. The gum will hold the fragments of Polyzoa firmly and safely in position, quite long enough before dissolving, so as to allow the specimens to be well cleaned: when this is done allow the slide to lie in the water until all the specimens are dissolved off from the surface; they can afterwards be collected with a soft hair-pencil, and dried upon blotting-paper, when they are quite ready for mounting.

A new form of section-cutting machine for general microscopical purposes, by Mr. H. F. Hailes, is described in the "Transactions of the Quekett Microscopical Club" (July, p. 243), to which those interested are referred, as an abstract of the paper would be useless without the drawings of the instrument.

Technology.—The Clothworkers' Company of London having, with a wise and truly patriotic munificence, founded and endowed a Department of Textile Industry in connection with the Yorkshire College of Science at Leeds, Mr. J. Beaumont, the "instructor" to the Department, along with Mr. McLaren,—of the firm of Smith and McLaren, of Keighley,—were commissioned to visit the weaving and other technical schools of France, Belgium, and Germany. The result of their observations has been embodied in a Report, which is published by Rivingtons, and which we commend to the heedful attention of all interested in the manufactures of this country. The following passage, which, *mutatis mutandis*, applies to all branches of manufactures, is an excellent answer to a common objection. It is said by many that if a young man wants to learn weaving, let him go to a first-rate mill. "The answer is that the weaving school is not meant to supplant the training received in the mill, but to supplement it in that particular where the latter fails. The apprentice in a mill will see other men designing and arranging new patterns; but will he learn to design and arrange them himself, to calculate the warp and weft required to weave them, or to cut the cards or arrange the healds? In a well-managed factory, where everything goes as if by clockwork, no one has time to teach a learner these things. *Even if there were the time, there might not be the desire to teach.* The jealousy of overlookers is often so great that, instead of helping a person who comes to learn, they not unfrequently do much to hinder him." We can confirm this statement from our own observation in such branches of industry as have come under our notice. The very object of apprenticeship is, in these days, to create and uphold a monopoly of practical knowledge. The fee is received, and as little as possible is given in return. Full testimony is borne to the striking success of the Polytechnic School of Aix-la-Chapelle. "Though very large and complete, another building of equal size is being erected by its side, which is to form the chemical department." There are now five hundred students, with twenty-four professors and eighteen assistants. The following passage may supply food for reflection:—"We noticed a number of packing-cases which we were told contained models of English patent machines, sent as a present by the British Government at the request of the Prince Imperial of Germany. We would suggest that the Government should be invited to extend its liberality to the (Leeds) College of Science, and to similar institutions in their own country." We might ask whether such international courtesies should not be made dependent on reciprocity and on international patent-right? At present it seems rather hard upon the English inventor whose application for a patent in Germany has been in due course refused to have his models sent abroad, and therefore the more conveniently pirated.

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